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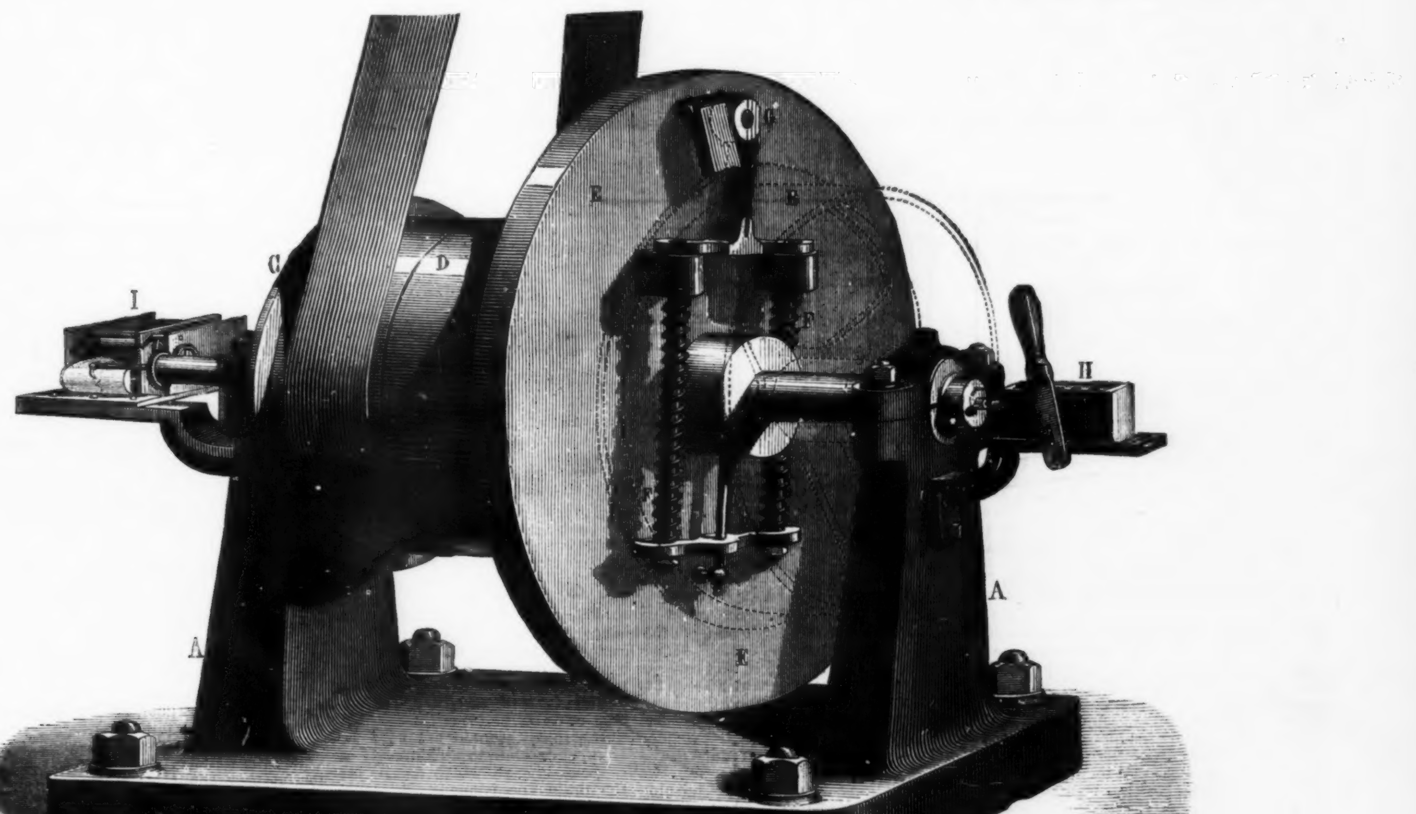
MORIN'S ROTARY DYNAMOMETER.

The apparatus ordinarily used for measuring the work performed by a motor is Prony's brake, and the results obtained by this are very accurate. For measuring the work of machines actuated by a motor, dynamometers with flexible plates are employed, but they are costly and difficult of manipulation. M. J. Morin, then, has just rendered a genuine service to all manufacturers by contriving a simple and practical apparatus for the purpose of measuring the work of machines used either in small workshops or large manufactories.

This new dynamometer (the illustration and description of which we borrow from the *Revue Industrielle*) consists of a flat cast iron disk mounted on an independent axle, and carrying a system of springs designed for receiving the effect transmitted by a motor machine. The power is transmitted by means of an endless belt and a shifting pulley on the shaft of the instrument, but which moves the flat disk through the intervention of a steel ribbon. When the machine is set in motion, the tension of the belt is transmitted first to this ribbon, which acts directly upon the system of springs. The flat disk is then moved along with it, and a pulley mounted on the other end of the shaft moves in its turn, by means of a belt, any machine whatever whose resistance is to be measured. On referring to the figure (in which the pulley commanded by the motor is denoted by dotted lines), the function of every part will be readily understood.



MORIN'S DYNAMOMETER.

Thus, A is the frame or support of the apparatus.

B, the pulley which receives its movement from the motor.

C, the pulley which transmits the movement to the machine of which it is desired to estimate the work absorbed.

D, a shifting pulley which allows the dynamometer to be stopped without stopping the motor.

E, a flat cast iron disk, carrying the steel ribbon and the system of springs.

F, the springs influenced by the resistance.

G, the pin and steel blade which transmit the power.

H, apparatus for measuring the number of revolutions.

I, a register or a totalizer.

The effort transmitted by the motor is observed in this way: A plate of steel which compresses the springs, and to which is fixed the ribbon, is fastened to a rack parallel to the plane of the flat disk, and engaging with a pinion fixed to the interior of the axle, the latter being hollowed out both transversely and in the direction of its axis. With the same pinion there engages a second rack, at right angles to the first, and prolonged by a rod which extends out at the extremity of the axle, and marks the movements of the spring upon a divided scale.

We thus know the effort, expressed in kilogrammes, that is exerted at the tangential point of the steel ribbon and the flat disk.

As we have just seen, an apparatus for marking the number of revolutions is placed at one of the extremities of the axle of the apparatus, and an automatic register, placed at the other extremity of the same axle, traces upon an unrolling paper ribbon a diagram of the work produced and employed.

One of these two appendices may be replaced by a totalizer, to make known, after any length of movement whatever, the power furnished by the motor, or the resistance offered by the machine actuated by it.

Morin's dynamometer resolves, as simply as it does correctly, every question relating to the force developed by all motor machines, as well as the work absorbed by the machines which are actuated by them. Two great advantages connected with this new apparatus deserve to be particularly noticed. The first is, that it measures the work, not only at any given moment while the machine is in movement, but it continues to do so during the whole time the machine is in operation, thus allowing all the variations in work occasioned by accidental causes to be ascertained. The second advantage is, that of avoiding the stoppage of all the machines in operation (as required by the use of all other known dynamometers) when it is desired to observe the dynamometric effects.

This apparatus will occupy a place, then, that has never as yet been filled in experimental mechanics, and there will be no doubt as to its success. Its very low price places it within the reach of all classes of buyers, and its solidity as well as its simplicity permits of its being placed in any hands, even in those of employes who have had little mechanical experience.

PROPOSED BRIDGE OVER THE ENGLISH CHANNEL.

A Paris correspondent of the London *Standard* says:

In order, therefore, to give as clear an idea as possible of this gigantic enterprise, it will, perhaps, be advisable, after stating that the viaduct is to span the Channel from Cape Grisnez to Folkestone, to begin at the beginning, or, rather at the bottom of the sea. According to the Admiralty soundings, the greatest depth of water to be found on the passage is 55 meters, and this only for a distance of some 4 kilometers, about half way between the Varne Rock and the French coast. This Varne Rock and its neighbor, the Calbart Reef, are destined to play an important part in the scheme before us. The former, situated at 15 kilometers from Folkestone and 20 kilometers from Cape Grisnez, is some 4 kilometers broad, covered with no more than from 2 to 15 meters of water. Being of solid rock, and in direct line with the projected viaduct, it offers itself as a natural half way resting place. The rock has till now constituted one of the greatest dangers to the navigation of the channel. M. De Sainte Anne, however, proposes not only to turn it to account by using it as the foundation for a portion of the viaduct, but also, in conjunction with the Calbart Reef, for the construction of a free port, in which vessels of the greatest tonnage will be able to seek shelter from the storms so frequent in the strait which separates England from France.

Both for the construction of this port and for reducing the depth of the water to 20 meters in those places where he will be obliged to construct his columns, M. De Sainte Anne proposes to adopt the method employed in the construction of the Cherbourg breakwater, which consists in dropping huge masses of iron into the sea, and in consolidating them by means of Roman cement. Knowing, however, how continually the Cherbourg breakwater was destroyed by the force of the waves, it may be permissible to doubt whether M. De Sainte Anne will find it so easy as he imagines to carry out the construction of his port of refuge in mid-channel; but the objections which apply to the superstructure of the breakwater for the port do not, however, hold good when the same system is employed to reduce the depth of water. On the foundations thus established it is intended to raise solid masses of masonry to some 40 meters above the level of the sea.

This is, of course, a gigantic work, the immensity of which will be seen at a glance when it is remembered that M. De Sainte Anne does not contemplate attempting in his viaduct any span exceeding 200 meters. The distance from Folkestone to Cape Grisnez being 35 kilometers, it will, therefore, be necessary to construct, at the very least, 175 immense blocks of masonry on which to place the superstructure.

The details of the scheme are not yet decided upon. At the present moment M. De Sainte Anne confines himself to the assertion that it is practically demonstrated by the Cherbourg breakwater that it is possible to find a solid foundation for as many columns as it may be ultimately found necessary to erect to support the viaduct. As to the superstructure itself, he proposes to employ three systems. On the Varne Rock and at the two extremities, where the water is shallow and the exigencies of navigation permit, he proposes to construct solid stone arches which will have noth-

ing to fear from the fiercest tempest. This massive masonry is to be followed by the girder bridge system, such as is employed in the Charing Cross Railway bridge. But to span the deep water he has recourse to the tubular bridge system as applied by Sir Robert Stephenson in the erection of the Menai bridge, and, quite recently, by the Americans on a much grander scale for the bridge between New York and Brooklyn.

With these three systems combined, the eminent originator of this gigantic scheme believes that he is not only certain to succeed in crossing the Channel, but also in satisfying the demands of every Government concerning the precautions to be taken to prevent the navigation of the English Channel being rendered even more dangerous than it is at present. Taking the Bretagne, which bears higher masts than any other vessel in the world, he has already applied to the French Admiralty to know if it considered that this vessel would experience any inconvenience in having to pass under his viaduct raised to 35 meters above the level of the sea. The answer he received was, that with 150 meters between the pillars and the platform of the viaduct raised to the altitude of 35 meters, neither the Bretagne nor any other vessel would be impeded in its passage.

Supposing this scheme to be practicable, there still remains much to be done before even the works can be commenced. M. De Sainte Anne promises, however, to commence operations without delay. He informs us that he will not require more than six or seven months for the scientific experiments and the thorough elaboration of his project, and that a sum of 1,000,000 francs will suffice to pay the preliminary expenses. To raise these funds he has already laid his project before the Chambers of Commerce of France and Belgium, from 84 of which he has already received adhesions. M. De Sainte Anne, nevertheless, wishing to give England the option of participating in the honor attached to the achievement of his project, intends shortly to visit its shores in order to lay the matter before the Government, and to submit his plan to the approbation of the technical societies.

HOUSE DRAINAGE.*

The first principle in house drainage is, that there ought never to be any constant bad odor connected with it. If there be such, it is an indication that *there is a defect somewhere*. Occasional offensive smells also usually reveal imperfect workmanship, incorrect methods, bad ventilation, or some failing in the quality of materials used, or in the proper working of some of the various parts.

As the different means of allowing escape of sewer gas into dwellings frequently exist for a long time without being detected, and as people may become so habituated to the daily presence of bad smells as not to notice them, it is evident that as much of the plumbing and of the soil pipes as possible should be *readily accessible to frequent inspection*, to allow of the application at once of the proper remedy for each defect. Inside the house, the drains should be of iron with flange or well tempered lead joints, and not be so laid under the cellar floor that they cannot be seen.

The danger to a healthy, vigorous person from breathing foul air, so far as the specific diseases are concerned, is much less than is commonly supposed; yet it is a risk of so great an injury that it should, of course, be avoided. If the foul air comes from a general sewer system, especially when the sewers are so badly constructed as many in our cities, or if from defective drains, allowing filth to accumulate and putrefy, the danger is ordinarily much greater than from filth before decomposition has begun; and the sense of smell, too, does not so constantly furnish a warning of its presence.

For a temporary purpose, and especially to arrest decomposition and destroy bad odors, disinfectants serve a good purpose; but they are simply palliatives at best, because they *cannot be efficiently applied* directly *to all places* where filth is likely to accumulate; and they should be depended upon only when the radical measures of prompt and effective removal of all filth, with thorough ventilation, cannot be adopted. Chloride of zinc (Burnett's disinfecting fluid), one part to four hundred of water, and carbolic acid, one part to a hundred, kill the *known* low organisms (*fungi, bacteria, infusoria*) immersed in them, and in that proportion are probably thorough disinfectants;† but, of course, the concentration must be increased according to the amount of filth in the fluid to be disinfected.

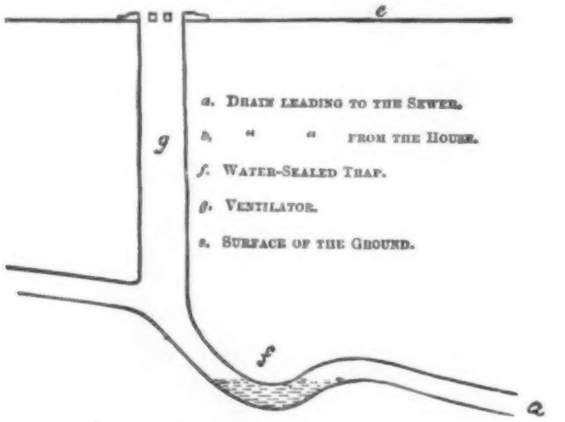
Drains should be of such a size (not over six inches in diameter for an ordinary house) and shape (round) as to concentrate the flow of drainage and prevent deposits; smooth inside, with continuous lines, free from offsets or jogs at the joints, of suitable inclination (one foot slope in twenty-four will usually be the least that is safe, unless a flush tank is used), and properly connected with the soil pipes at one end and the sewer at the other, strong, and of durable material (glazed earthenware or iron). They should be used for all *liquid* refuse, but never for garbage or ashes; and no filth or dirty water should be thrown out in back yards, except to be taken up at once by vegetation; and it should never be allowed to run in gutters in the streets. The common defects in drains cannot all be mentioned here, but are fully discussed in articles by E. S. Philbrick, C.E., and E. C. Clarke, C.E., in the seventh and tenth reports* of the Board.

As a defective house drain may affect not only the occupants of the house, who may not be owners (and many of whom may not be even tenants), but even a whole neighborhood, every house drain at least, and better, also, soil pipes and plumbing, should be constructed and arranged according to approved plans, and be under municipal inspection and control. A plan of the work should always be put on record, both for future use and because its preparation will insure some forethought and care in its adaptation to the requirements of house and sewer—and particularly as neither owner nor mechanic can commonly prepare such plans with accuracy and nicety, without calling for advice on some one skilled in that work. It would be well to have two copies of ground plan and profile; one to be kept at the house, and the other with the officers in control of the sewer department. The plans should contain *all the works projected*, in connection with the supply of air † and water to the house, together with the apparatus for removing the water, after its use, from sinks, water closets, bath rooms, etc., through soil pipes, drains, traps, etc.; and this plan should include *grades* also of all important parts (cellar, drains, catch basins, yards, sewer).

Drainage of wet sites for houses is also of very great importance, as well as drainage to remove filth; when needed, the same principles are involved and the same processes applied to its application as in agricultural drainage. Tile drains, from two to five inches in diameter, with a fall of not less than one foot in one hundred, are the best for that purpose; they should be laid at least six inches—but better two feet—below the level of the bottom of the cellar. They should never be used to carry away kitchen slops, or indeed anything except the water from the soil and subsoil. They should be placed about twenty feet apart in tough soil or clay, and from that distance to forty feet apart in gravel, etc. *Damp cellars are injurious to health;* they often produce consumption, pneumonia, rheumatism, neuralgia, and predispose to diphtheria, and other diseases, although strong, vigorous persons frequently do not feel their immediate influence upon themselves, and it is not always felt upon their children. [See a paper by Hon. H. F. French, in the fourth annual report of the Board.]

Few sewers are so well constructed as not to require to be isolated from houses by traps. An excellent method of securing this result is shown by the cut, Fig. 1. The ventilator, *g*, may be made of brick and cement as a manhole, with arrangements for packing, to prevent freezing in winter, and excellent traps are in the market, in one piece, to be used in that case; or it may be continued to the roof by a pipe, which should be used only when there is a trap between it and the sewer. The current of air will usually be found downward, and then upward, and out through the soil pipe. If extended to the roof it should not have its upper extremity near a window, or close to a chimney. If used also as a rain water spout—a plan which has its advantages for flushing the drains occasionally—it would be well to have another vent from the drain to allow the escape of compressed air; *provided*, the soil pipe and rain water spout are ever used for discharging water at the same time.

Fig. 1.



The best arrangement of the soil pipe is to carry it up through the roof. In houses already fitted with pan closets, and not more than two to each soil pipe, sufficient ventilation may often be got by a pipe at least two inches in diameter, the top of which should not be near and at the level of windows or chimneys. A soil pipe may be well ventilated, too, by a pipe passing into a *constantly heated* chimney, provided it be so arranged as not to conduct "sewer gases" into any other part of the house, either by down draughts through other flues, fire places, etc., or by insecure joints, and contraction and expansion of the pipe from changes of temperature. If there are several points of discharge into a single soil pipe, there should be a long vent pipe similar to that described on p. 453 of the seventh report of the Board, or a vent into the external air from each, to prevent "siphoning out" of traps.

Water closets, sinks, basins, and bath tubs should be so securely trapped as to prevent the admission of foul air. Generally speaking, water closets should be ventilated by direct communication with the external air, and they should never be in sleeping rooms. A ventilator directly under the seat of the water closet, and passing, with the precautions already given, into a chimney, serves a very good purpose in old houses, when the arrangements cannot be made to fulfill the best requirements, or wherever a constant draught of air can be maintained through the closet door and out through the ventilator. It is well not to place water closets in cellars.

Of the various patterns of water closets in use, the smoothest, the simplest, and those easiest to keep *clean*, *well trapped*, *fully ventilated*, and *thoroughly flushed*, are the best.* Many are now in use which do not fulfill these indications, but there are others in which the faults of the pan closet and of the old ill flushed hopper closet are corrected; whichever are used, the bowls should be often washed with soap and water.

Wash basins should not be in sleeping rooms, unless protected by *efficient* traps; and even then it is safer to guard against a possible accident, and have them in an adjoining room.

In view of the fact that many cities have appointed boards of health, by virtue of the authority conferred upon them by Chap. 133 of the Acts of the General Court of 1877, the Board desire to call their attention to the following important section of that Act:

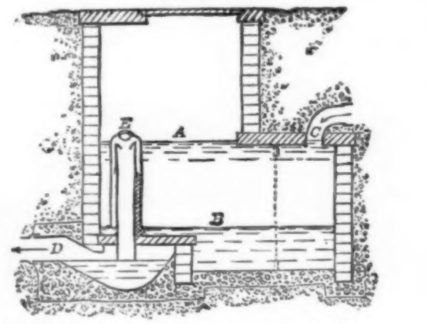
"Section 5. *Said boards of health, and the board of health of the city of Boston, in addition to the powers conferred upon them by existing statutes, are hereby authorized to prepare and enforce, in their respective cities, such regulations as they may deem necessary for the safety and health of the people, with reference to house drainage, and its connection with public sewers, where such connection is made.*"

Gross defects in house drainage may be detected, and minor defects may be commonly found, by dropping a half ounce of oil of peppermint into the soil pipe, at its opening above the roof, or through the topmost sink or water closet in the house; a few quarts of water—better hot—should be poured down after it. If *another* person than the one who has used the peppermint visits the various rooms, cellar, closets, etc., there generally will be no difficulty in ascertaining where a leak may be. A better method still, but very costly, is to use hydrostatic pressure, as is the custom in testing gas pipes. In any case, the opinion of an inspector or person familiar with the matter is desirable.

When houses are left vacant for a time, the traps are apt to become dry—an evil which should be avoided, so far as is possible, by flushing them from time to time, and always a few days before reoccupation of the house; and this flushing of the traps should be supplemented by free ventilation with open windows.

Where water closets are used, and there are no sewers, the best disposal of the sewage is by the flush tank and irrigation under the surface of the soil, as described on p. 334 of the seventh, p. 135 of the eighth, and p. 11 of the ninth annual report of the Board. (See Fig. 2.†) If cesspools must be used, they should be tight, and often emptied by the odorless process, or else have their contents pumped out on the surface of the ground for fertilizing purposes, where that can be done without causing a nuisance. If the sewage is placed on the soil in the morning of a dry, clear day, when the sun is shining, and in places where it may be readily absorbed by the earth, the odors from it are the least offensive. In very loose soil, and remote from dwellings, ordinary loose walled cesspools may be used without danger for a time; but the custom cannot often be entirely approved. An overflow into a stream or upon the land from a tight walled cesspool often creates a nuisance—a difficulty which has been simply and successfully provided for by means of sub-surface irrigation, through porous drainage pipes from the top of the cesspool, laid in the same general way as for use with the flush tank.

Fig. 2.



Sewers are of the first importance for removal of sewage, where the water carriage system is adopted. When for any reason they cannot be introduced, the greatest consideration should be used before it is decided to introduce water closets, if the result must be to drench the soil with filth and water by means of loose walled cesspools. The water carriage system, however, in the opinion of the Board, *if sewers and house drainage are planned and constructed as they should be*, is by far the most satisfactory, both from a sanitary point of view and as a civilizing agent; and even where the sewers are very defective, the house drains may often be so isolated from them by traps and good ventilation as to make the evils of water carriage less than those of any other system, provided there is an abundant water supply.

In some cases, where the soil has been polluted so as to endanger the wells, and a public water supply is not practicable, rain water may be stored for use; but it should be filtered and kept free from contamination by dust, dirt, drainage, etc. The water first coming down in rain collects impure matter from the atmosphere and from the roofs of houses, and this should be thrown away. The rest may be filtered through a brick wall renewed every three months, or by means of animal charcoal, quartz sand, spongy iron,

* From a recent circular of the Massachusetts State Board of Health.

† Three pounds of green copperas and one pint of carbolic acid to a pail of water may be used as a cheap and useful palliative of filth which cannot be promptly removed.

* These reports may be found in most of the libraries in the State, or upon application to the board of selectmen.

† It occasionally happens that a defect in a drain communicates with the air-supply to a furnace, and so is the means of distributing foul air over the house; of course the two should never be so near together that such an accident can be possible.

* Directions with reference to these points may be had on application to the sanitary engineers intrusted with the drainage of houses; as it is not the purpose of the Board to enter into details, but to insure proper care and forethought in matters so vital to the health of the community.

† Reprinted by kind permission of Messrs. Harper & Brothers, from an article in the June number of the "New Monthly Magazine," by Col. G. E. Waring, jun., who has also drained the town of Lenox on this principle. "A is the surface of the water when the tank is full, and B when it is emptied. The capacity of the tank between the lines A and B is about five barrels. In front of the entrance there is a wire screen to prevent the passage of coarse material. This is held in place by wooden wedges, and may easily be removed for cleansing. The depression below the line B is for the accumulation of solid matters which may not become decomposed. A portion of the tank is carried up to the surface of the ground, with a movable cover for a man-hole. E is Field's automatic annular siphon, by which the tank is emptied as soon as its contents rise high enough to flow over the top of its inner (and longer) limb. The short limb is a dome inclosing the inner limb, with a water-way all around its bottom, reaching to the line B. The drainage flows into the flush-tank, where it is held until the top of the siphon is reached. The whole amount (five barrels) is then discharged with great rapidity into the main sewer (D), washing it clean from end to end. The flow of sewage alone is sufficient to remove all accumulations from the sewer." See also pp. 295–298, 482, 483, and 52?, of the *second edition* of Mr. Baldwin Latham's Sanitary Engineering.

sponge, cotton flannel, etc., *frequently cleaned*, although no one of these methods is so good as filtration through clean, well aerated soil [See "Household Filtration" in an article by Prof. W. Ripley Nichols, in the ninth report of the Board, pp. 205 *et seq.*]

Where the constant system of supply is so universally used as in this country, cisterns for drinking water are seldom depended upon except for rain water. Any overflow pipe from them should always be kept from discharging directly or through a rain water spout into a drain or sewer, because such an arrangement would serve as a means of conducting "sewer gas" into the cistern; and a trap would not be in such case a sufficient protection.

Important as are the proper construction and maintenance of soil pipes and drains, their thorough usefulness depends also upon a sewerage system adjusted to the wants of each city and town where water carriage is adopted. Sewers should always be laid according to a definite plan embracing the whole area to be sewered. They should be skillfully built, smooth inside, in straight lines, suitably ventilated, adapted for ready and thorough inspection, of proper size, shape, grade, etc.; they should be tight, and so constructed as not to allow percolation of filth through their walls into the soil and air; although, of course, they may properly be porous enough to drain the soil of superfluous moisture. *The sewage should start from the houses, and go in a continuous current, without allowing any deposits or stopping, until it reaches its destination before putrefaction has begun.* Details with reference to this matter may be found in the eighth report of the Board, pp. 139 *et seq.*, in an article by E. S. Chesbrough, C.E.

EXPLOSION OF THE FLOURING MILLS AT MINNEAPOLIS, MINNESOTA, MAY 2, 1878, AND THE CAUSES OF THE SAME.

By S. F. PECKHAM.

As I was sitting at the tea table on the evening of May 2, I was startled by a noise that sounded as if something as heavy as a barrel of flour had been tipped over on the floor above. A few seconds later the sound was repeated, and we all ran to the door which commanded a full view of the falls and manufacturing portion of the city. An immense volume of black smoke enveloped the spot where the Washburn A Mill had stood, and a perpendicular column of smoke was projected into the air above the elevator at least four hundred feet. The Humboldt and Diamond Mills were directly behind the elevator from the place where I stood. A heavy wind was blowing from a point a little to the east of north, a direction from the Washburn A Mill toward the elevator and the other two mills. In less than two minutes from the time of the first explosion, the elevator, which was 108 feet high, was wrapped in flames from top to bottom. If the structure had been saturated in oil the flames could not have spread much more rapidly. In five minutes flame and smoke were pouring from every window in the Day and Rollins, Zenith and Galaxy Mills, which were between the Washburn A Mill and the river, producing a conflagration which from ordinary causes would not have gained such headway in two hours. Six flouring mills, the elevator, a machine shop, blacksmiths' shop, and planing mill, with a number of empty and loaded cars, were in flames in five minutes from the time fire was first observed by any one who survived the disaster.

From my own point of observation, which was about a mile distant, but two distinct explosions were heard; others nearer heard three, the first not as violent as the other two; while those nearer still heard in addition a sound which they described as a succession of sharp hisses, resembling the sound of burning gunpowder. Those observers to the windward whose attention was arrested by the light produced, beyond the distance of half a mile, heard only one or two reports, or failed to hear any report at all. From all the testimony in reference to sound it appears that the blow upon the air was not sufficiently sudden to produce a penetrating sound, but rather a dull, heavy blow, which was not communicated laterally to any great distance.

Burning wheat or flour was smelled for several minutes before the explosion by persons in such a position that the wind would carry the odor to them. Smoke was also seen issuing from what was known as the exhaust flour dust spout of the Washburn A Mill for several minutes preceding the explosion.

At the instant the explosion occurred all observers agreed that the Washburn A Mill was brilliantly illuminated from basement to attic. The illumination was reflected from the water at and around the falls in such a manner as to remind one observer of the effect of a brilliant sunset. Another compared it to the reflection of sunlight from windows when the sun is near the horizon. Still another, who was crossing the lower bridge, had his attention called to what appeared to be a stream of fire, which, as he described it, issued from a basement window and went back again. Immediately thereafter each floor above the basement became brilliantly illuminated, the light appearing simultaneously at all the windows, only an appreciable interval of time intervening as the stories ignited one after the other. Then the windows burst out, the walls cracked between the windows and fell, and the roof was projected into the air, followed by an immense volume of smoke and flame which ascended to an estimated height of from six to eight hundred feet. As the column of smoke was expanded and borne off upon the wind, brilliant flashes resembling lightning passed to and fro.

Two men, so near the Humboldt Mill that they were nearly buried by the falling rubbish, and on the opposite side from the Washburn A Mill, heard a loud report distinctly while the walls of the Humboldt Mill were still standing, and at the same time were knocked down. Immediately after they saw flames issuing from the basement windows of the Humboldt Mill, and at the same instant, before they could regain their feet, they experienced a second shock, and miraculously escaped being buried beneath the falling walls.

The enormous and sudden displacement of air that followed the explosion, and the tremendous force which was consequently exerted laterally, was shown in the condition of the round house of the Chicago, Milwaukee, and St. Paul railroad, and the broken windows in all directions. The round house was a wooden structure about forty or fifty feet from the Diamond Mill. The sills were drawn out toward that mill until the building burst, letting a part of the roof fall in and leaving the sides standing at a sharp angle. Ordinary windows, and those of strong plate-glass on Washington avenue, one-fourth of a mile distant, were projected into the street. Not only the glass, but the sash went out bodily, particularly in the lower stories of the buildings. Persons on the river at the water's edge noticed a displacement of the water, producing a wave estimated to be eighteen inches high, before they heard the report of the explosion.

Whole sheets of the corrugated iron with which the elevator was covered, measuring eight by two feet, but quite thin, were picked up on the east side of the river, more than two miles distant, and pieces of six-inch flooring, from two to ten feet long, were carried to intermediate points.

An examination of the ruins of the several buildings showed that the walls of the Humboldt Mill lay upon those of the Diamond Mill, and those of the Diamond Mill upon those of the west end of the Washburn A Mill, showing that the buildings did not explode simultaneously but successively. The Washburn A Mill evidently exploded first from fire originating within it, and the high wind prevailing at the time carried the *flame* into the adjoining mills to the south, and away from the mills next the river. There was enough burning middlings and flour thrown through the broken windows of the latter mills to set them on fire, but they did not explode. Some significance may attach to the fact that the three mills exploded were all running with more or less open French middlings purifiers, while the three that did not explode had been shut down for several days. There is no question but that the French purifiers project a great deal more dust into the atmosphere of the mills than those that are inclosed, but I have no doubt that in *any* flouring mill sufficient dust accumulates upon beams and machinery to produce an explosive atmosphere if from any cause this dust is scattered into the air and flame is communicated to the mixture while the dust is suspended.

There was less than a barrel each of lard oil, lubricating oil, and high-test kerosene in the Washburn A Mill at the time of the explosion.

There is absolutely no proof that any explosive material other than is produced in the manufacture of flour from wheat, was in any one of the buildings destroyed, in the cars around them, or in the neighborhood. The testimony of millwrights conclusively showed that fire produced by heated bearings is of such extremely rare occurrence in flouring mills as to practically exclude such a cause.* No suspicion of incendiarism has ever been expressed.

A slight fire, the effects of which were in no wise serious, occurred in the Washburn A Mill about three months before the explosion. It was discovered from the outside of the mill that smoke was issuing from a spout or conductor that discharged the air that was drawn through between the stones. The object for which the air is drawn through is to cool the stones and to carry off the vapor produced from the wheat by the use of temperature due to friction. In this case the effects of *fire* were traced back from the outside of the building to one of the sets of stones on the north side of the mill used for grinding middlings. The effects of *flame*, however, did not extend beyond the blower which produced the exhaust. This led to the conclusion that the fire did not enter the dust house, although the smoke must have passed through it. It is supposed that the fire was caused by friction between the stones, they having run dry from one of the causes that may produce dry stones.

In answer to inquiries made of several millers in the Minneapolis mills, I found them uniformly of the opinion that the meal or flour as it left the stones had a temperature of about 100° F. or less. A number of careful experiments, made with an ordinary chemical thermometer, showed that the wheat enters the stones from the driers at a temperature of fully 100° F., and that it leaves the stones at 120°—130° F. The temperature of the ground middlings as it left the stones averaged about ten degrees higher.

It was also the concurrent testimony of millers and mill owners that dry stones are of comparatively frequent occurrence, and that they are practically unavoidable. I am convinced that in the Washburn A Mill the frequency of danger from dry stones was considerably increased in consequence of the large number of stones in the mill, and especially from the fact that so few men were employed having the immediate oversight of the stones. Only two men were employed at the same time for the forty-two run of stone, a number inadequate for that supervision which so important a matter demands, as it is impossible from the large space occupied by so many stones, and the noise incident to their action, that even with the usual signals employed dry stones should be detected as soon as they become a source of danger.

Obstruction of the feed from any one of a number of accidental causes will produce dry stones. The danger arises from the friction of the stones heating the last portion of the grist that remains between the stones to a temperature sufficient to char it, or convert it into a substance resembling tinder, which would readily ignite from a spark produced by the stones striking together. Another source of danger arises from nails or gravel passing between the stones with the grist and increasing the friction, producing either a rise of temperature or a train of sparks; perhaps both.

I am aware that numerous instances of dry stones can be cited that have proved perfectly harmless. An instance is on record in which a run of stone ground each other all night with no other result than the complete removal of the grooves which gave the stones a cutting face. On the other hand, cases have occurred in which the grooves became filled with charred wheat of a dark brown color, packed into them so solidly as to require a mill pick for its removal. It requires no argument to show that this tinder thus formed would become ignited from a train of sparks that would inevitably follow contact of the stones as the grist became compacted or completely removed from between them. It was found by experiment† that masses of flour that had become heated and charred, ignited readily and smouldered, but were inflamed with considerable difficulty, but it should be borne in mind that a number of sets of these stones are connected with a common spout or conductor, through which a strong current of air is being continually drawn, and which is filled with a dense cloud of very fine particles of starch (chiefly) heated to a maximum temperature of 140° F. Experiment also proved that the proper mixture of flour dust and air would not burn explosively except when brought in contact with *flame*. White-hot wires and glowing charcoal only burned the particles in contact with them. But it was found that burning pellets of charred wheat and flour would ignite wood, which a strong draught of air readily fanned into a blaze. Under the conditions previously stated, with a draught of air passing through the dry stones strong enough to convey the pellets of smouldering tinder into the common wooden conductor, an explosion becomes possible.

It is urged that these conductors are damp from condensed moisture, and also that a large amount of moisture escapes from the wheat and is conveyed away by the current of air. This loss is, no doubt, correctly estimated at from five to six per cent. It is, however, chiefly during the first grinding of the raw wheat that this loss is experienced. The middlings is drier, is ground at a higher temperature, and is ground finer, producing more dust. The higher temperature renders the material more inflammable, and at the same time insures a more complete solution of the vapor in the current of air. Moreover, the first fire in the Washburn A Mill was traced directly to a set of stones which ground nothing but middlings, and all that is known concerning the origin of the fire that produced the explosion confirms the supposition that that fire originated in a set of stones on the opposite side of the mill, which was one of six sets, all of which were used exclusively for grinding middlings, discharging into a common spout or conductor which communicated directly with a dust house in which the dust settled to the amount of several hundred pounds a day. An explosion in this conductor, communicating *flame* to the dust house, would scarcely fail to cause the successive explosions of the dust house and the different stories of the mill, the shock of the first explosion being sufficient to throw the dust of the mill into the air.

The opinion expressed by one of the witnesses at the inquest, "that stones are liable to run dry at any time by accident," and that "dry stones can hardly be avoided by any amount of foresight," appears to be generally entertained by millwrights, millers, and mill owners. Let it be granted that all experience shows that ninety-nine per cent. of dry stones injures nothing but the stones themselves, the one per cent. of residue is burdened with fearful possibilities. If dry stones cannot be prevented in small mills, where one miller has charge of perhaps six run of stone, the danger is more than proportionally increased in a mill where one man has charge of twenty run, both with reference to prevention and detection. The problem, therefore, for the consideration of parties immediately interested is how to prevent or detect dry stones, particularly those used for grinding middlings. This practical problem appears to be fundamental, and one compared with which all others are without much importance. It is true that but few millers are without their experience of minor explosions or flashes resulting from careless use of lanterns or open lights. Indeed, I have been profoundly impressed with the generally innocent reputation of flouring mills when considered in the light of the immense number of accidents well known to millers and insurance companies; a number surprisingly large if confined to those occurring in the States of Minnesota and Wisconsin within a few years past. The remedy in such cases is so obvious that the most ordinary care and intelligence is sufficient.—*American Journal of Science.*

* These gentlemen concurred in the statement that the spindle which carries the stone had been known to become *welded* into the socket in which it revolved, stopping the stone. When asked if the friction produced a welding heat, one replied: "No, nowhere near it." It must be an example of perfect metallic contact, producing cohesion.

† Experiments made by Prof. L. W. Peck before the coroner's jury.

ON A NEW METHOD OF PREPARING GELATINO-BROMIDE OF SILVER.

By DR. VAN MONCKHOVEN.*

IN order to make what follows perfectly clear, let me first shortly recapitulate the ordinary method of preparing emulsions of gelatino-bromide of silver. I will take the method of Mr. Bennett, as it serves for a type of all others. Take 20 grammes of gelatine and 7 grammes of ammonium bromide, and dissolve them in 250 grammes of water heated to 32° C.; then add 11 grammes of silver nitrate, also dissolved in 250 grammes of water, at the same temperature, and stir well. This mixture is allowed to rest for several days (always being kept at a temperature of 32° C.), or for a still longer time, in proportion as an emulsion is to be produced more or less sensitive to light. It is then cooled until it assumes a gelatinous consistency, well washed to remove all traces of ammonium nitrate and all excess of alkaline bromide which it may still contain, and flowed over the plates, which are then put aside to dry. The latter are exposed in a camera, and then developed by what is known as the alkaline method.

This process I have studied with great care, and have observed the following phenomena:

1. If, at the instant of preparation, the emulsion be flowed over a glass plate, the film will be seen to be nearly transparent, notwithstanding the presence of the silver bromide which it contains in a finely-divided state. The film is white and opalescent; in looking through it the bars of a window or any other object which is strongly illuminated will be plainly distinguished. But if the plate be coated with emulsion the day after the latter has been prepared the film will be much less transparent, and if the emulsion be used a week after its preparation a perfectly-opaque film will be obtained. Of course, it is understood that the emulsion must be well shaken before flowing it over the plate.

2. On examining the color of the freshly-prepared emulsion, it will be seen to be a milky white; but at the end of a week it will be found to have turned a distinctly greenish white.

3. When films prepared with emulsions in different states of emulsification—as described under 1—are exposed to the light, those which are fresh and are white and transparent will assume the slaty blue color characteristic of the haloid salts of silver more quickly than the others. The older films, possessing a green tint, will scarcely darken any further on direct exposure to the light.

4. What renders this latter experiment still more curious is the following fact, which is, so to say, its antithesis: White films, which change most rapidly under exposure to direct light, are slower to receive an impression in the camera, while the green films, which scarcely alter at all in direct daylight, are incomparably more rapid in the photographic instrument. From the circumstance, therefore, of a silver salt blackening rapidly in direct light, it cannot be inferred that it will behave in the same way in the camera.

I have the honor of exhibiting to the society prepared plates in different stages of emulsification. The first ones, marked No. 1, are white and transparent, darken quickly in the light, but are very slow in the camera; the others, marked No. 2, are of a green tint, and opaque; they do not become darker in the light, and are eight times as rapid in the camera as the former.

In order to obtain an insight into the phenomenon which I have above described, I submitted the bromide of silver in its different molecular conditions to a long course of experiments, of the result of which I will now give an account.

* Read before the Photographic Society of France at the meeting of the 1st of August last.

Silver bromide exists in two states:* in the first, which we will call the ordinary state, since it is well known to chemists, it is obtained by pouring a solution of alkaline bromide into one of an excess of silver nitrate acidulated with nitric acid, and then well shaking. By this means there is thrown down a heavy precipitate, which soon lumps together; it has a greenish-white color, and turns gray on exposure to the light. The other modification is formed by dissolving this green bromide in ammonia, and then adding an acid; a bromide of a white color and milky consistency is thus obtained, very light, and passing with ease through the finest filter. But if this milky fluid be allowed to stand for some days it will deposit at the bottom of the test-glass a white bromide, which may be more or less easily collected on a filter, and this bromide, on being exposed to the light, assumes the violet tint of the haloid salts of silver. Lastly, when this white bromide is left to itself for a long time it becomes heavy and granular, and turns a greenish color—in short, assumes the molecular condition of the green bromide.

These facts go to prove that emulsificatien consists in transferring into the green bromide the white bromide which has been formed in the substance of the gelatine. The particles of bromide are at first of extreme fineness; they have a tendency to reunite with larger particles, and then pass into the state of the green bromide—not heavy and curled, but fine. What also tends to prove this is the fact that the emulsion at first possesses all the characteristics of the white bromide; afterward, when it has ripened, it has the same properties as the green bromide. The transparency of the film at the outset shows that the bromide is in a finely subdivided state. Later on the particles become larger, and hence the opacity of the film to light. In many cases, also, the particles are visible under the microscope.

Carbon, when subdivided, gives similar phenomena. Take, for instance, some lampblack, and beat it up in a solution of gelatine. A little of this liquid spread in a thin film over glass appears at first opaque, but in proportion as the churning is continued and the particles rendered finer, the film will become translucid, and finally transparent. Indian ink, which consists of carbon so finely subdivided that there is no deposit from its aqueous solution, is quite transparent, if a thin film of the solution be examined.

If, therefore, my views be correct, the greater the facility offered to the particles of bromide to coalesce the quicker will be the emulsification. This is what happens when ammonia is added, according to Mr. Bennett's formula; the addition of ammonia transforms the white bromide into green bromide in as many hours as it would otherwise require days. On the other hand, the addition of a few drops of sulphuric acid retards the emulsification. The latter is also effected much more rapidly in solutions containing a small quantity of gelatine than in these which are more concentrated. The former are more liquid and mobile, and consequently permit the fine particles of white bromide better to coalesce into the larger particles of the green bromide.

According to this theory of emulsification, we can lay down the most favorable conditions for the preparation of gelatino-bromide. The great defect of Mr. Bennett's method, and of those which have succeeded it, is due to the decomposition of the gelatine during emulsification, and to the long time required for washing the preparation. When the gelatine has been kept hot for any length of time it does not adhere to the glass, or if it do adhere, it does so very slowly. It often happens, in consequence, that, after developing, the whole film curls up, and even detaches itself from the plate. Disasters of this kind arise from the gelatine decomposing. Moreover, the operation of washing is outrageously long and tedious, and requires an enormous quantity of water.†

The following method avoids these inconveniences: I prepare very pure and dilute hydrobromic acid, and I determine accurately the amount of it required to precipitate exactly 10 grammes of silver nitrate. I then dissolve this quantity of acid in 200 grammes of water, with which I incorporate by heating 2½ grammes of gelatine. On the other part—and from this moment I entirely operate in the dark room—I precipitate 10 grammes of silver nitrate by a very slight excess of bicarbonate of soda; I let it settle for twenty-four hours, and then renew the water to the same amount, after which I let it settle again previous to decanting. On this precipitate of silver carbonate I pour a hot solution of 2 grammes of gelatine in 200 grammes of water. This is well stirred, and then I pour on it the solution of gelatine and hydrobromic acid. The mixture is thoroughly shaken every quarter of an hour, and is kept at the constant temperature of 50° C. The silver carbonate dissolves slowly in the hydrobromic acid, and the silver bromide is formed in the colloidal liquid in a state of extreme subdivision. At the end of ten or twelve hours the mixture, when flowed over glass plates, has a greenish white color. I next introduce 10 grammes of gelatine, cut into very thin shreds, which I dissolve by stirring, and then, without washing the emulsion, I flow it over the glass plates

In order to obtain a success with this method it is necessary to take some precaution. The hydrobromic acid must be free from phosphorus and sulphur; the water used for washing the silver carbonate must contain no trace of carbonic acid. This paper has, however, already exceeded its proper limits; I will, therefore, reserve the more minute details for a further note, which I intend to publish subsequently.

In an emulsion prepared by this method there is always an excess of hydrobromic acid and of silver carbonate, but I have satisfied myself by other experiments that the presence of these substances does not affect the results. This is not the case if carbonate be replaced by the oxide of silver; the emulsion is then gray, and gives rise to fogging. The plates that I have prepared by this method are twenty times as rapid as the best wet collodion, and, compared with the best English plates, I have found them to be three or four times as rapid. For the rest, the same observations and the same methods apply also to collodio-bromide.—*Photographic News.*

VINEGAR has been found to attack pure tin as well as its alloys with lead, the quantity of metal dissolved increasing with the proportion of lead present. Alloys of tin and lead, to which 4 per cent. of antimony had been added, were likewise attacked and had entered into solution.

* In a subsequent note I intend to return to this subject and the observations of M. Stas on the same.

† Captain Abney has recently made public a method in which he forms the bromide separately. He washes it, and then mixes it with the gelatine. He asserts that after a few hours the bromide is distributed throughout the mass. I have repeated this experiment accurately according to the directions, and have also caused it to be repeated by others. On each occasion the film was completely granular and full of large particles of the silver bromide, forming so many white points, which in the image were transparent. Time will show whether Captain Abney's method is practicable.

[Continued from SUPPLEMENT, No. 193.]

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING OF 1879, AT SARATOGA SPRINGS, N. Y.

ON THE IDENTITY OF THE LINES OF OXYGEN WITH BRIGHT SOLAR LINES, AS SHOWN IN PHOTOGRAPHS TAKEN WITH INCREASED DISPERSION.

By PROF. HENRY DRAPER.

Read before the American Association, Saratoga Springs, 1879.

I INTEND in this paper to speak of the steps that led to the discovery of oxygen in the sun, to describe very briefly some of the successive improvements of the electrical and optical apparatus employed, and finally to discuss the earlier results and to show their subsequent confirmation.

In 1857, after the meeting of the British Association at Dublin, some of the members, by the kindness of the Earl of Rosse, were invited to visit the 6-foot reflector at Birr Castle. In this way I enjoyed the advantage of seeing the methods by which that great instrument had been produced, and, on returning to America in 1858, it prompted me to begin the construction of a metallic speculum of 15½ inches aperture. Soon after, by the advice of Sir John Herschel, who had early information of Foucault's work in Paris, the metal was abandoned in favor of silvered glass, and several mirrors were ground and polished. The telescope was constructed especially for photography, and good results were obtained in 1863, culminating in the production of a photograph of the moon 50 inches in diameter. These were published in the Smithsonian Contributions to Science for the succeeding year. The success procured with this instrument prepared the way for making a silvered glass equatorial of 28 inches aperture, which was ready for use in 1871, though it has been much modified since. It was obvious that increased light-collecting power and precise equatorial movements were necessary for the modern applications of physics to astronomy. More recently still there has been attached to the same equatorial stand an achromatic telescope of 12 inches aperture, made by Alvan Clark & Sons, this being particularly intended for solar spectroscopic work.

Soon after the 28-inch reflector was turned to stellar and planetary photographic spectroscopy it became evident that the results obtained required for their interpretation photographs of metallic and non-metallic spectra, so that comparisons might be instituted leading to precise knowledge of the elements producing lines at the more refrangible end of the spectrum. This led to a division of the work into two parts, one for the observatory in the country in the warmer half of the year, the other for my town laboratory during the winter. It was in the latter that most of the oxygen work has been done, and consequently the engine, the Gramme machine, the induction coil, and the large spectroscope are generally there.

My first photographs of metallic spectra were taken with such apparatus as happened to be at hand, viz., a couple of Bunsen's batteries, an induction coil giving a spark of one-half inch, and a Hofmann's direct-vision spectroscope. The length of the spectrum from G to H was about half an inch, but, though the dimensions were small, the promise was great. After some experiments, however, and after obtaining more powerful instrumental appliances, it seemed best, as able physicists were engaged on the metallic spectra, to turn attention more particularly to photographing the spectra of the non-metals. The exceedingly valuable researches of Dr. Huggins had brought the astronomical importance of nitrogen, carbon, and hydrogen into notice, and these accordingly were next the subject of experiment. Not long after, on examining a series of photographs of the fluted spectrum of nitrogen with juxtaposed solar spectra, the suspicion that there was a coincidence of some bright bands in the two spectra was suggested. On pursuing the subject with more and more powerful electrical and optical arrangements, the coincidence of bright lines of oxygen with bright lines in the solar spectrum was discovered.

The original apparatus, as has been said above, was on a very small scale, but it was soon replaced by a larger battery, a 2-inch induction coil, and a direct-vision prism of 1 inch aperture by Browning. The electrical part was made more and more powerful as the research proceeded, the 2-inch induction coil being succeeded by one of 6 inches, and that in turn by a Ruhmkorff coil, capable of giving a spark of 17 inches. The battery was eventually superseded by a Gramme dynamo-electric machine which can produce a current powerful enough to give, between carbon points, a light equal to 500 standard candles. When this machine is properly applied to the 17-inch induction coil it will readily give 1,000 10-inch sparks per minute. These, being condensed by fourteen Leyden jars, communicate an intense incandescence to air, and light enough is produced to permit of the use of a narrow slit, and of a collimator and telescope of long focus.

Since 1877, when the first publication of the discovery of oxygen in the sun was made, still further improvements, especially in the optical parts, have been completed, so that I am now enabled to photograph the oxygen spectrum with four times the dispersion then employed. For the sake of clearness, it is best to give a brief description: 1st, of the electrical part; and 2d, of the optical part.

The electrical part consists of the Gramme machine and its driving engine, the induction coil, the Leyden jars, and the terminal or spark compressor. An advantage the Gramme has over battery is in the uniformity of the current it gives when a uniform rate of rotation of its bobbin is kept up. Of course this implies the use of a prime mover that is well regulated. The petroleum engine of one and a half horse power I have employed is convenient and safe and does this duty well. As to the Gramme itself, it is only needful to call attention to a modification of the interior connections. In one form the bobbin of wire which revolves between the magnets is double, so that the current produced may be divided into two. Under ordinary circumstances, where the machine is used to produce light, both sides of the bobbin send their currents through the electro-magnets. But if the whole current be sent through a quick-working break circuit into an induction coil, the electro-magnets do not become sufficiently magnetized to produce any appreciable effect. It is expedient, therefore, to arrange the connections so that one-half of the bobbin gives a continuous current through the electro-magnets and keeps up the intensity of the magnetic field, and then the current from the other half of the bobbin may be used for exterior work, whether continuous or interrupted.

At first a Foucault mercurial interruptor was arranged to make and break the current passing into the primary circuit of the induction coil; but during the past year, by carrying the rate of rotation of the Gramme up to 1,000 per minute, the strength of the current has been so much increased that the mercury was driven violently out of the cup, and hence it was essential to arrange a mechanical break in which solid metal alone was used. This has been accomplished by fastening on the axis of the Gramme bobbin a wheel with an interrupted rim, which serves the purpose well.

As to the induction coil, it is only needful to say that it gives a good thick spark, which is limited to twelve inches to avoid the risk of injuring the insulation. The Leyden jars are fourteen in number, having altogether seven square feet of coating on each surface.

The arrangement of the terminals from the Leyden jars to get the steadiest and brightest effects has offered great difficulties. The condensed spark taken in the open air or in a gas under atmospheric pressure pursues, if unconfined, a zigzag course, and this is apt to produce a widening of the lines in the photographed spectrum. But, after many experiments, it turned out that the spark might be compressed between two plates of thick glass, or, better yet, between two plates of soapstone. If the interval between the plates was directed toward the slit of the spectroscope the lateral flickering of the spark was prevented, and yet at the same time the spark was freely exposed to the slit without the intervention of glass or any substance on which the volatilized metal from the terminals could deposit. Very early in this research it had become apparent that Plücker's tubes could not be employed with electrical currents of more than a certain intensity, partly on account of the deposit that took place in the capillary portion, and partly because the terminals became so hot as to melt and crack the glass. Moreover, it was desirable to use one terminal of iron, so as to be sure that the spectrum of the gas was correctly adjusted to the solar spectrum, and this is impracticable with Plücker's tubes. An additional advantage arises from the soapstone plates, viz., the temperature of the small volume of air between the terminals is materially increased, and increased brightness results. I have tried the effect of warming the air by passing it through a coil of brass tube maintained at a bright red heat, but this does not seem to make any perceptible difference when the terminals are inclosed in the spark compressor.

The optical part of my apparatus has undergone many modifications. At first a Hofmann direct-vision prism was combined with a lens of six inches focus; this was soon after replaced by a Browning direct-vision prism and a lens of eighteen inches focus, the latter being arranged for conjugate foci, so that it was virtually as if collimating and observing lenses of thirty-six inches focus were employed. The final system, perfected this winter, consists of a collimator of two inches aperture and twenty-six inches focus, succeeded by two bisulphide of carbon prisms of two inches aperture and an observing or photographing lens of six feet six inches focal length. These prisms belong to Mr. Rutherfurd, and are the same he made for producing his celebrated solar prismatic spectrum. This gives a dispersion of about eight inches between G and H, and enables me to get original negatives on a scale about half the size of Angström's charts in the 'Spectre Normal du Soleil.' When it is remembered that the light produced by the electric current in the spark compressor is scarcely equal to one standard candle, it will be realized that this great dispersion nearly attains the limit of present possibility. By comparison I have found, when the electric arc from this Gramme volatilizes iron, the light is sixty times stronger than the most vivid incandescence of air that I have produced.

The slit of the spectroscope is about one inch long, and opposite the lower half is placed a right-angled prism which serves to bring in a beam of sunlight from a heliostat. We thus have the solar spectrum and the air spectrum upon the plate at the same time, so that the two spectra on the negative are, strictly speaking, simultaneously produced. Moreover, by the aid of a magnifier we can ascertain, just previous to an exposure, whether the adjustments are in the best order. It is not commonly known that, to obtain the last degree of exactness in coincidence between a solar and an air spectrum, many precautions are necessary, and that is the reason it is desirable to have iron vapor present at one of the poles so as to determine the reliability of the coincidence by comparing iron in the spark spectrum with iron in the sun.

Having thus alluded to some of the principal peculiarities of the apparatus constructed for this research, it is proper in the next place to point out the nature of the evidence afforded by the photographs of the presence of oxygen in the sun. The first photographs were on so small a scale that they did not even give rise to a suspicion of this fact, and it was not until 1876 that I felt sufficiently sure to make any publication. At this time the original negatives were about two inches long from G to H, and they bore an enlargement of three or four times quite well. The Albertype printed in 1877 in *Nature*, the *Comptes Rendus*, and this journal, was produced from such an enlargement. Since that time, in order to meet the criticism that perhaps the dispersion was not sufficient to disclose the lack of coincidence if such existed, I have increased the dispersion four times, and am thus enabled to make enlarged photographs on a scale twice the size of Angström's chart. Enlargements of the juxtaposed spectra of air and sun on this scale are now presented for inspection.

Of course an enlargement never does justice to the original from which it was produced, and, in order to study the matter faithfully, the negative must be examined carefully with a magnifier. Besides this, owing partly to the fact that the solar spectrum has suffered from absorptive influences, both in the earth's atmosphere and in the solar atmosphere, the conditions under which the oxygen spectrum is seen when compared with the spark spectrum are modified. In fact, a critical study of the two spectra demands that each line of oxygen should be separately photographed with the corresponding region of the sun's spectrum, so as to reproduce as nearly as possible the same conditions for each. As an instance of the modifications which may be caused by the solar atmosphere, the superposition of absorption lines on the bright lines of oxygen may be mentioned. If, as seems to be the case, the stratum giving the oxygen spectrum in the sun lies deeper than the reversing layer in which iron exists, I see no reason why an iron absorption line, for instance, may not fall upon an oxygen bright band. In support of this supposition that oxygen is photospheric, it may be stated that, though I have examined the chromosphere on many occasions, I have not as yet seen the bright oxygen lines project beyond the apparent limb of the sun as observed in the spectroscope, although several of the chromosphere lines catalogued by Young were readily visible. On consulting with Professor Young, he expressed the opinion that the oxygen groups near G did not appear as bright lines in the chromosphere, even under the exceptionally favorable circumstances he enjoyed at Sherman. For the purpose of continuing the study of this point, and also of

examining small areas on the sun, faculæ, spots, etc., Alvan Clark & Sons are constructing a special spectroscope for me which can give the dispersion of twenty heavy flint prisms and can bear high magnifying power.

If it be conceded that there are bright lines in the spectrum of the solar disk, which seems to be the opinion of several physicists, and especially Lockyer, Cornu, and Hennessy, the question of their origin naturally attracts attention. It seems that there is great probability, from general chemical reasons, that a number of the non-metals may exist in the sun. The obvious continuation of this research is in that direction. But the subject is surrounded by exceedingly great obstacles, arising principally from the difficulty of matching the conditions as to temperature, pressure, etc., found in the sun. Any one who has studied nitrogen, sulphur, or carbon, and has observed the manner in which the spectrum changes by variations of heat and pressure, will realize that it is well nigh impossible to hit upon the exact conditions under which such bodies exist at the level of the photosphere. The fact that oxygen, within a certain range of variation, suffers less change than others of the non-metals has been the secret of its detection in the sun. It appears to have a greater stability of constitution, though Schuster has shown that its spectrum may be made to vary. I have already begun an extended series of experiments on the non-metals; but the results exhibit such confusion that their bearing cannot at present be distinctly seen. In the case of nitrogen the broad bands between G and H exhibit, under the most intense incandescence, a tendency to condense into narrow bands or lines, and indeed there are some sharp lines of nitrogen in the photographs now presented.

It does not follow, therefore, that the bright bands of oxygen are necessarily the brightest parts of the solar spectrum. Other substances may produce lines or bands of greater brilliancy.

There is also another cause for a difference of appearance in a bright-line spectrum produced in a laboratory and bright lines in the sun. While the edges of a band in the spark spectrum may be nebulous or shaded off, the corresponding band in the solar spectrum may have its edges sharpened by the action of adjacent dark lines due to one or another of the metallic substances in the sun.

On the whole, it does not seem improper for me to take the ground that, having shown by photographs that the bright lines of the oxygen spark spectrum all fall opposite bright portions of the solar spectrum, I have established the probability of the existence of oxygen in the sun. Causes that can modify in some measure the character of the bright bands of the solar spectrum obviously exist in the sun, and these, it may be inferred, exert influence enough to account for such minor differences as may be detected.

In closing, it may be well to give some idea of the amount of labor and time this research has already consumed, and this cannot be better done than by a statement of the production of electrical action that has been necessary. Each photograph demands an exposure of fifteen minutes, and, with preparation and development, at least half an hour is needed. The making of a photograph, exclusive of intermediate trials, requires, therefore, about 30,000 10-inch sparks—that is, 30,000 revolutions of the bobbin of the Gramme machine. In the last three years the Gramme has made 20,000,000 of revolutions. The petroleum engine only consumes a couple of drops of oil at each stroke, and yet it has used up about 150 gallons. Each drop of oil produces two or three 10-inch sparks. It must also be borne in mind that comparison spectra can only be made when the sun is shining, and clouds therefore are a fertile source of loss of time.

[Continued from SUPPLEMENT No. 193.]

HISTORY AND METHODS OF PALÆONTOLOGICAL DISCOVERY.

By PROFESSOR O. C. MARSH, President.

IT is important to bear in mind, too, that Cuvier's preparation for the study of the remains of animals was far in advance of any of his predecessors. He had devoted himself for years to careful dissections in the various classes of the animal kidgdom, and was really the founder of comparative anatomy, as we now understand it. Cuvier investigated the different groups of the whole kingdom with care, and proposed a new classification founded on the plan of structure, which in its main features is the one in use to-day. The first volume of his Comparative Anatomy appeared in 1800, and the work was completed in five volumes, in 1805.

Previous to Cuvier, the only general catalogue of animals was contained in Linnæus' "Systema Naturæ." In this work, as we have seen, fossil remains were placed with the minerals, not in their appropriate places among the animals and plants. Cuvier enriched the animal kingdom by the introduction of fossil forms among the living, bringing all together into one comprehensive system. His great work, "Le Règne Animal," appeared in four volumes in 1817, and with its two subsequent editions remains the foundation of modern zoology. Cuvier's classic work on vertebrate fossils —"Recherches sur les Ossemens Fossiles," in four volumes, appeared in 1812–13. Of this work, it is but just to say that it could only have been written by a man of genius, profound knowledge, the greatest industry, and with the most favorable opportunities.

The introduction to this work was the famous "Discourse on the Revolutions of the Surface of the Globe," which has perhaps been as widely read as any other scientific essay. The discovery of fossil bones in the gypsum quarries of Paris, by the workmen, who considered them human remains; the careful study of these relics by Cuvier, and his restorations from them of strange beasts that had lived long before, is a story with which you are all familiar. Cuvier was the first to prove that the earth had been inhabited by a succession of different series of animals, and he believed that those of each period were peculiar to the age in which they lived.

In looking over his work after a lapse of three-quarters of a century, we can now see that Cuvier was wrong on some important points, and failed to realize the direction in which science was rapidly tending. With all his knowledge of the earth, he could not free himself from tradition, and believed in the universality and power of the Mosaic deluge. Again, he refused to admit the evidence brought forward by his distinguished colleagues against the permanence of species, and used all his great influence to crush out the doctrine of evolution, then first proposed. Cuvier's definition of a species, the dominant one for half a century, was as follows: "A species comprehends all the individuals which descend from each other, or from a common parentage, and those which resemble them as much as they do each other."

The law of "Correlation of Structures," as laid down by Cuvier, has been more widely accepted than almost anything else that bears his name; and yet, although founded in truth, and useful within certain limits, it would certainly lead to serious error if applied widely in the way he proposed.

In his discourse, he sums this law as follows: "A claw, a shoulder blade, a condyle, a leg or arm bone, or any other bone separately considered, enables us to discover the description of teeth to which they have belonged; so also reciprocally we may determine the form of the other bones from the teeth. Thus, commencing our investigation by a careful survey of any one bone by itself, a person who is sufficiently master of the laws of organic structure, may, as it were, reconstruct the whole animal to which that bone had belonged."

We know to-day that unknown extinct animals cannot be restored from a single tooth or claw, unless they are very similar to forms already known. Had Cuvier, himself, applied his methods to many forms from the early Tertiary or older formations, he would have failed. If, for instance, he had had before him the disconnected fragments of an Eocene Tillodont, he would undoubtedly have referred a molar tooth to one of his Pachyderms; an incisor tooth to a rodent; and a claw bone to a carnivore. The tooth of a Hesperornis would have given him no possible hint of the rest of the skeleton, nor its swimming feet the slightest clew to the ostrich-like sternum or skull. And yet, the earnest belief in his own methods led Cuvier to some of his most important discoveries.

Jean Lamarck (1744–1829), the philosopher and naturalist, a colleague of Cuvier, was a learned botanist before he became a zoologist. His researches on the invertebrate fossils of the Paris Basin, although less striking, were not less important than those of Cuvier on the vertebrates; while the conclusions he derived from them form the basis of modern biology. Lamarck's method of investigation was the same, essentially, as that used by Cuvier, namely: a direct comparison of fossils with living forms. In this way, he soon ascertained that the fossil shells embedded in the strata beneath Paris were, many of them, extinct species, and those of different strata differed from each other. His first memoir on this subject appeared in 1802,* and, with his later works, effected a revolution in conchology. His "System of Invertebrate Animals" appeared the year before, and his famous "Philosophie Zoologique," in 1809. In these two works, Lamarck first announced the principles of Evolution. In the first volume of his "Natural History of Invertebrate Animals,"† he gave his theory in detail; and to-day one can only read with astonishment his far-reaching anticipations of modern science. These views were strongly supported by Geoffroy Saint-Hilaire, but bitterly opposed by Cuvier; and their great contest on this subject is well known.

In looking back from this point of view, the philosophical breadth of Lamarck's conclusions, in comparison with those of Cuvier, is clearly evident. The invertebrates on which Lamarck worked offered less striking evidence of change than the various animals investigated by Cuvier; yet they led Lamarck directly to evolution, while Cuvier ignored what was before him on this point, and rejected the proof offered by others. Both pursued the same methods, and had an abundance of material on which to work, yet the facts observed induced Cuvier to believe in catastrophes; and Lamarck, in the uniform course of nature. Cuvier declared species to be permanent; Lamarck, that they were descended from others. Both men stand in the first rank in science; but Lamarck was the prophetic genius, half a century in advance of his time.

While the Paris Basin was yielding such important results for palæontology, its geological structure was being worked out with great care. The results appeared in a volume by Cuvier and Alex. Brongniart, chiefly the work of the latter, published in 1808.‡ This was the first systematic investigation of Tertiary strata. Three years later, the work was issued in a more extended form. The separate formations were here carefully distinguished by their fossils, the true importance of which for this purpose being distinctly recognized. This advance was not accepted without some opposition, and it is an interesting fact that Jameson, who claimed for Werner the theory here put in practice, rejected its application, and wrote as follows: "To Cuvier and Brongniart we are indebted for much valuable information in their description of the country around Paris, but we must protest against the use they have made of fossil organic remains in their geognostical descriptions and investigations."§

William Smith (1769–1839), "the father of English geology," had previously published a "Tabular View of the British Strata." He appears to have arrived independently at essentially the same view as Werner in regard to the relative position of stratified rocks. He had determined that the order of succession was constant, and that the different formations might be identified at distant points by the fossils they contained. In his later works, "Strata Identified by Organized Fossils," published in 1816–20, and "Stratigraphical System of Organized Fossils," 1817, he gave to the world results of many years of careful investigations on the secondary formations of England.

In the latter work he speaks of the success of his method in determining strata by their fossils, as follows:

"My original method of tracing the strata by the organized fossils embedded therein is thus reduced to a science not difficult to learn. Ever since the first written account of this discovery was circulated, in 1799, it has been closely investigated by my scientific acquaintances in the vicinity of Bath, some of whom search the quarries of different strata in that district, with as much certainty of finding the characteristic fossils of the respective rocks as if they were on the shelves of their cabinets."

The systematic study of fossils now attracted attention in England also, and was prosecuted with considerable zeal, although with less important results than in France. An extensive work on this subject, by James Parkinson, entitled "Organic Remains of a Former World," was begun in 1804, and completed in three volumes in 1811. A second edition appeared in 1833. This work was far in advance of previous publications in England, and, being well illustrated, did much to make the collection and study of fossils popular. The belief in the geological effects of the deluge had not yet lost its power, although restricted now to the later deposits, for Parkinson, in his later edition, wrote as follows:

* "Mémoires sur les fossiles des environs de Paris." 1802–6.

† "Histoire naturelle des animaux sans vertèbres." 7 vols. Paris, 1815–1822. Second edition. 11 vols. 1835–1845.

‡ "Essai sur la Géographie Mineralogique des environs de Paris." 4to, 1808.

§ Translation of Cuvier's Discourse. Note K (B), p. 108, 1817.

"Why the earth was at first so constituted that the deluge should be rendered necessary; why the earth could not have been at first stored with all those substances, and endued with all those properties, which seemed to have proceeded from the deluge; why so many beings were created, as it appears, for the purpose of being destroyed, are questions which I presume not to answer."

William Buckland (1784–1856) published in 1823 his celebrated "Reliquiæ Diluvianæ," in which he gave the results of his own observations in regard to the animal remains found in the caves, fissures, and alluvial gravels of England. The facts presented are of great value, and the work was long a model for similar researches. Buckland's conclusions were, that none of the human remains discovered in the caves were as old as the extinct mammals found with them, and that the deluge was universal.

In speaking of fossil bones found in the Himalaya Mountains, he says:

"The occurrence of these bones at such an enormous elevation in the region of eternal snow, and, consequently, in a spot now unfrequented by such animals as the horse and deer, can, I think, be explained only by supposing them to be of antediluvian origin, and that the carcasses of the animals were drifted to their present place, and lodged in sand by the diluvial waters."

The foundation of the "Geological Society of London," in 1807, marks an important point in the history of palæontology. To carefully collect materials for future generalizations was the object in view, and this organization gradually became the center in Great Britain for those interested in geological science. The society was incorporated in 1826, and has since been the leading organization in Europe for the advancement of the sciences within its field. The Geological Society of France, established at Paris in 1832, and the German Geological Society, founded at Berlin in 1848, have likewise contributed largely to geological investigations in these countries, and, to some extent, in other parts of the world. In the publications of these three societies, the student of palæontology will find a mine of valuable materials for his work.

The systematic study of fossil plants may be said to date from the publication of Adolphe Brongniart's "Prodrome," in 1828.* This was very soon followed by his larger work, "Histoire des végétaux fossiles," issued in 1828–48. Brongniart pursued the same method as Cuvier and Lamarck, viz., the comparison of fossils with living forms, and his results were of great importance.

In his "Tableau des genres végétaux fossiles," etc., published in Paris in 1849, he gives the classification and distribution of the genera of fossil plants, and traces out the historical progression of vegetable life on the globe, as he had done, to a great extent, in his previous works. He shows that the cryptogamic forms prevailed in the primary formations, the conifers and cycads in the secondary, and the higher forms in the Tertiary, while four-fifths of living plants are exogens.

In England, Lindley and Hutton published, in 1831–37, a valuable work in three volumes, entitled "Fossil Flora of Great Britain." This work was illustrated by many accurate plates, in which the plants of the coal formation were especially represented. Henry Witham also published two works in 1831 and 1833, in which he treated especially of the internal structure of fossil plants. "Antediluvian Phytology," by Artis, was published in London in 1838. Bowerbank's "History of the Fossil Fruits and Seeds of the London Clay" appeared in 1843.

Hooker's memoir "On the Vegetation of the Carboniferous Period, as Compared with that of the Present Day," published in 1848, was an important contribution to the science. Bunbury, Williamson, and others also published various papers on fossil plants. This branch of palæontology, however, attracted much less attention in England than on the Continent.

In Germany, the study of fossil plants dates back to the beginning of the century. Von Schlotheim, a pupil of Werner, published in 1804 an illustrated volume on this subject. A more important work was that of Count Sternberg, issued in 1820–38, and illustrated with excellent plates. Cotta, in 1832, published a book with the title "Die Dendrolithen," in which he gave the results of his investigations on the inner structure of fossil plants. Von Gutbier, in 1835, and Germar, in 1844–53, described and figured the plants of two important localities in Germany.

Corda's "Beiträge zur Flora der Vorwelt," issued at Prague in 1845, was essentially a continuation of the work of Sternberg. Unger's "Chloris protogæa," 1841–45, "Genera et species plantarum fossilium," 1850, and his larger work published in 1852, are all standard authorities. In the latter, the theory of descent is applied to the vegetable world. Schimper and Mougeot's "Monograph on the Fossil Plants of the Vosges," 1845, was well illustrated, and contained noteworthy results.

Göppert, in 1836, published a valuable memoir entitled "Systema Filicum Fossilium," in which he made known the results of his study of fossil ferns. In the same year this botanist began a series of experiments, in which he attempted to imitate the process of fossilization as found in nature. He steeped various animal and vegetable substances in waters holding some calcareous, others silicious, and others metallic matter in solution. After a slow saturation, the substances were dried, and exposed to heat until the organic matters were burned. In this way Göppert successfully imitated various processes of petrifaction, and explained many things in regard to fossils that had previously been in question.

His discovery of the remains of plants throughout the interior of coal did much to clear up the doubts about the formation of that substance. In 1841 Göppert published an important work, in which he compared the genera of fossil plants with those now living. In 1852 another extensive work by this author appeared, entitled "Fossile Flora des Uebergangs-Gebirges."

Andræ, Braun, Dunker, Ettingshausen, Geinitz, and Goldenberg, all made notable contributions to fossil botany in Germany during the period we are now considering.

The systematic study of invertebrate fossils, so admirably begun by Lamarck, was continued actively in France. The Tertiary shells of the Seine valley were further investigated by Defrance, and especially by Deshayes, whose great work on this subject was begun in 1824.† Desmoulins's essay on "Sphérulites," in 1826, Blainville's memoir on "Belemnites," in 1827, Férussac's various memoirs on land and fresh water fossil shells, were valuable additions to the subject. A later work of great importance was D'Orbigny's "Paléontologie Française," 1840–44, which described the

* "Prodrome d'une histoire des végétaux fossiles." 8vo. Paris, 1828.

† "Description des cognilles fossiles des environs de Paris." 3 vols. Paris, 1824–37.

mollusca and radiates in detail, according to formations. The other publications of this author are both numerous and valuable.

Brongniart and Desmarets's "Histoire naturelle des Crustaces Fossiles," published in 1822, is a pioneer work on this subject. Michelins' memoir on the fossil corals of France, 1841–46, was another important contribution to palæontology. Agassiz's works on fossil echinoderms and mollusks are valuable contributions to the science. The works of D'Archiac, Coquand, Cotteau, Desor, Edwards, Haime, and De Verneuil are likewise of permanent value.

In Italy, Bellardi, Merian, Michellotti, Phillipi, Zigno, and others contributed important results to palæontology.

In Belgium, Bosquet, Nyst, Koninck, Ryckholt, Van Beneden, and others have all aided materially in the progress of the science.

In England, also, invertebrate fossils were studied with care, and continued progress was made. Sowerby's "Mineral Conchology of Great Britain," in six volumes, a systematic work of great value, was published in 1812–30, and soon after was translated into French and German. Its figures of fossil shells are excellent, and is still a standard work. Miller's "Natural History of the Crinoidea," published at Bristol, in 1821, and Austin's later monograph, are valuable for reference. Brown's "Fossil Conchology of Britain and Ireland" appeared in 1839, and Brodie's "History of the Fossil Insects of England," in 1845. Phillips' illustration of the geology of Yorkshire, 1829–36, and his work on the Palæozoic fossils of Cornwall, Devonshire, and West Somerset, 1843, contained a great deal of original matter in regard to fossil remains.

Morris' "Catalogue of British Fossils," issued in 1843, and the later edition, in 1854, is most useful to the working palæontologist. The memoirs of Davidson on the Brachiopoda, Edwards, Forbes, Morris, Lycett, Sharpe, and Wood on other Mollusca, Wright on the Echinoderms, Salter on Crustacea, Busk on Polyzoa, Jones on the Entomostraca, and Duncan and Lonsdale on Corals, are of especial value. King's volume on Permian fossils, Mantel's various memoirs, Dixon's work on the fossils of Sussex, 1850, and McCoy's works on Palæozoic fossils, all deserve honorable mention. Sedgwick, Murchison, and Lyell, although their greatest services were in systematic geology, each contributed important results to the kindred science of palæontology during the period we are reviewing.

In Germany, Schlotheim's treatise, "Die Petrifactenkunde," published at Gotha in 1820, did much to promote a general interest in fossils. By far the most important work issued on this subject was the "Petrifacta Germanica," by Goldfuss, in three folio volumes, 1826 to 1844, which has lost little of its value. Bronn's "Geschichte der Natur," 1841–46, was a work of great labor, and one of the most useful in the literature of this period. The author gave a list of all the known fossil species, with full references, and also their distribution through the various formations. This gave exact data on which to base generalizations, hitherto of comparatively little value.

Among other early works of interest in this department may be mentioned Dalman's memoir on "Trilobites," 182[illegible], and Burmeister's on the same subject, 1843. Giebel's well known "Fauna der Vorwelt," 1847–1856, gave lists of all the fossils described up to that time, and hence is a very useful work. The "Lethæa Geognostica," by Bronn, 1834–38, and the second edition by Bronn and Roemer, 184[illegible]–56, is a comprehensive general treatise on palæontology, and the most valuable work of the kind yet published.

The researches of Ehrenberg, in regard to the lowest forms of animals and plants, threw much light on various points in palæontology, and showed the origin of extensive deposits, the nature of which had before been in doubt. Von Buch, Barrande, Beyrich, Berendt, Dunker, Geinitz, Heer, Hörnes, Klipstein, Von Münster, Reuss, Roemer, Sandberger, Suess, Von Hagenow, Von Hauer, Zeiten, and many others, all aided in the advancement of this branch of science. Angelin, Hisinger, and Nilsson, in Scandinavia; Abich, De Waldheim, Eichwald, Keyserling, Kutorga, Nordman, Pander, Rouillier, and Volborth, in Russia; and Pusch, in Poland, published important results on fossil invertebrates.

The impetus given by Cuvier to the study of vertebrate fossils extended over Europe, and great efforts were made to continue discoveries in the direction he had so admirably pointed out.

Louis Agassiz (1807–73), a pupil of Cuvier, and long an honored member of this association, attained eminence in the study of ancient as well as of recent life. His great work on "Fossil Fishes"* deserves to rank next to Cuvier's "Ossemens Fossiles." The latter contained mainly fossil mammals and reptiles, while the fishes were left without a historian till Agassiz began his investigations. His studies had admirably fitted him for the task, and his industry brought together a vast array of facts bearing on the subject.

The value of this grand work consists not only in its faithful descriptions and plates, but also in the more profound results it contained. Agassiz first showed that there is a correspondence between the succession of fishes in the rocks and their embryonal development. This is now thought to be one of the strongest points in favor of evolution, although its author interpreted the facts as bearing the other way.

Pander's memoirs on the fossil fishes of Russia form a worthy supplement to Agassiz's classic work. Brandt's publications are likewise of great value; and those of Lund, in Sweden, have an especial interest to Americans, in consequence of his researches in the caves of Brazil.

Croizet and Jobert's "Recherches sur les Ossemens fossiles du département du Puy de-Dôme," published in 1828, contained valuable results in regard to fossil mammals. Geoffroy St. Hilaire's researches on fossil reptiles, published in 1831, were an important advance. De Serres and De Christol's explorations in the caverns in the south of France, published between 1829 and 1839, were of much value. Schmerling's researches in the caverns of Belgium, published in 1833–36, were especially important on account of the discovery of human remains mingled with those of extinct animals.

Deslongchamps's memoirs on fossil reptiles, 1835, are still of great interest. Pictet's general treatise on palæontology was a valuable addition to the literature, and has done much to encourage the study of fossils.† De Blainville, in his grand work, "Ostéographie," issued in 1839–56, brought together the remains of living and extinct vertebrates, forming a series of the greatest value for study. Aymard and Pomel's contributions to vertebrate palæontology are both of value. Gervais and Lartet added much to our knowledge of the subject, and Bravard and Hébert's memoirs are well known.

* "Recherches sur les Poissons fossiles," 1833–45.

† "Traité élémentaire de paléontologie," etc., Genève. 4 vols. 1844–46. Second edition. Paris, 1853–55.

The brilliant discoveries of Cuvier in the Paris Basin excited great interest in England, and when it was found that the same Tertiary strata existed in the south of England, careful search was made for vertebrate fossils. Remains of some of the same genera described by Cuvier were soon discovered, and other extinct animals new to science were found in various parts of the kingdom. König, to whom we owe the name *Ichthyosaurus*, and Conybeare, who gave the generic designation *Plesiosaurus*, and also *Mosasaurus*, were among the earliest writers in England on fossil reptiles. The discovery of these three extinct types, and the discussion as to their nature, forms a most interesting chapter in the annals of palæontology.

The discovery of the *Iguanodon*, by Mantell, and the *Megalosaurus*, by Buckland, excited still higher interest. These great reptiles differed much more widely from living forms than the mammals described by Cuvier, and the period in which they lived soon became known as the "age of reptiles." The subsequent researches of these authors added largely to the existing knowledge of various extinct forms, and their writings did much to arouse public interest in the subject.

Richard Owen, a pupil of Cuvier, followed, and brought to bear upon the subject an extensive knowledge of comparative anatomy, and a wide acquaintance with existing forms. His contributions have enriched almost every department of palæontology, and of extinct vertebrates especially he has been, since Cuvier, the chief historian. The fossil reptiles of England he has systematically described, as well as those of South Africa. The extinct Struthious birds of New Zealand he has made known to science, and accurately described in extended memoirs. His researches on the fossil mammals of Great Britain, the extinct Edentates of South America, and the ancient Marsupials of Australia, each forms an important chapter in the history of our science.

The personal researches of Falconer and Cautley in the Sivalik Hills of India brought to light a marvelous vertebrate fauna of Pliocene age. The remains thus secured were made known in their great work, "Fauna Antiqua Sivalensis," published at London in 1845. The important contributions of Egerton to our knowledge of fossil fishes, and Jardine's well known work, "Ichnology of Annandale," also belong to this period.

The study of vertebrate fossils in Germany was prosecuted with much success during the present period. Blumenbach, the ethnologist, in several publications between 1803 and 1814, recorded valuable observations on this subject. In 1812 Sömmering gave an excellent figure of a pterodactyl, which he named and described. Goldfuss' researches on the fossil vertebrates from the caves of Germany, published in 1820–23, made known the more important facts of that interesting fauna. His later publications on extinct amphibians and reptiles were also noteworthy.

Jäger's investigations on the extinct vertebrate fauna of Würtemberg, published between 1824 and 1839, were an important advance. To Plieninger's researches in the same region, 1834–44, we owe the discovery of the first Triassic mammal (*Microlestes*), as well as important information in regard to Labyrinthodonts. Kaup's researches on fossil mammals, 1832–41, brought to light many interesting forms, and to him we are indebted for the generic name *Dinotherium*, and excellent descriptions of the remains then known.

Count Münster's "Beiträge zur Petrifactenkunde," published 1843–46, contained several valuable papers on fossil vertebrates; and the separate papers by the same author are of interest. Andreas Wagner wrote on Pterosaurians in 1837, and later gave the first description of fossil mammals of the Tertiary of Greece, 1837–40. Johannes Müller published an important illustrated work on the Zeuglodonts, in 1849, and various notable memoirs; and Quenstedt, interesting descriptions of fossil reptiles, as well as other papers of value. Rütimeyer's suggestive memoirs are widely known.

Hermann von Meyer's contributions to vertebrate palæontology are by far the most important published in Germany during the period we are now considering. From 1830, his investigations on this subject were continuous for nearly forty years, and his various publications are all of value. His "Beiträge zur Petrifactenkunde," 1831–33, contains a series of valuable memoirs. His "Palæologica," issued in 1832, includes a synopsis of the fossil vertebrates then known, with much original matter. His great work, "Zur Fauna der Vorwelt," 1845–60, includes a series of monographs invaluable to the student of vertebrate palæontology. This work, as well as his other chief publications, was illustrated with admirable plates from his own drawings. Other memoirs by this author will be found in the "Palæontographica," of which he was one of the editors. In the many volumes of this publication, which began in 1851, and is still continued, will be found much to interest the investigator in any branch of palæontology.

The "Palæontographical Society of London," established in 1847, has also issued a series of volumes containing valuable memoirs in various branches of palæontology. These two publications together are a storehouse of knowledge in regard to extinct forms of animal and vegetable life.

It may be interesting here to note briefly the use of general terms in palæontology, as the gradual progress of the science was indicated to some extent in its terminology. At first, and for a long time, the name "*fossil*" was appropriately used for objects dug from the earth, both minerals and organic remains. The term "Oryctology," having essentially the same meaning, was also used for this branch of study. For a long period, too, the termination *ites* (λίθος, a stone) was applied to fossils to distinguish them from the corresponding living forms; as, for instance, "*Ostracites*," used by Pliny. At a later date, the general name "figured stones" (*lapides figurati*) was extensively used; and less frequently, "Deluge stones" (*lapides diluviani*). The term "organized fossils" was used to distinguish fossils from minerals, when the real difference became known, although the name "*Reliquiæ*" was sometimes employed. The term "petrifactions" (*Petrificata*) was defined by John Gesner in his work on fossils in 1758, and was afterwards extensively used. Palæontology is comparatively a modern term, having come into use only within the last half century. It was introduced about 1830, and soon was generally adopted in France and England, but in Germany it met with less favor, though used to some extent.

It would be interesting, too, did time permit, to trace the various opinions and superstitions, held at different times, in regard to some of the more common fossils, for example, the Ammonite or the Belemnite; of their supposed celestial origin; of their use as medicine by the ancients, and in the East to-day; of their marvelous power as charms, among the Romans, and still among the American Indians. It would be instructive, also, to compare the various views expressed by students in science, concerning some of the stranger extinct forms, for instance, the Nummulites, among Protozoa; the Rudistes, among Mollusks; or the Mosasaurus, among reptiles. Dissimilar as such views were, they indicate in many cases gropings after truth—natural steps in the increase of knowledge.

The third period in the history of palæontology, which, as I have said, began with Cuvier and Lamarck at the beginning of the present century, forms a natural epoch extending through six decades. The definite characteristics of this period, as stated, were dominant during all this time, and the progress of palæontology was commensurate with that of intelligence and culture.

For the first half of this period, the marvelous discoveries in the Paris Basin excited astonishment and absorbed attention; but the real significance and value of the facts made known by Cuvier, Lamarck, and William Smith were not appreciated. There was still a strong tendency to regard fossils merely as interesting objects of natural history, as in the previous period, and not as the key to profounder problems in the earth's history. Many prominent geologists were still endeavoring to identify formations in different countries by their mineral characters, rather than by the fossils embedded in them. Such names as "Old Red Sandstone" and "New Red Sandstone" were given in accordance with this opinion. Humboldt, for example, attempted to compare the formations of South America and Europe by their mineral features, and doubted the value of fossils for this purpose. In 1823, he wrote as follows: "Are we justified in concluding that all formations are characterized by particular species? that the fossil shells of the chalk, the muschelkalk, the Jura limestone, and the Alpine limestones, are all different? I think this would be pushing the induction much too far."* Jameson still thought minerals more important than fossils for characterizing formations; while Bakewell, later yet, defines palæontology as comprising "Fossil Zoology and Fossil Botany, a knowledge of which may appear to the student as having little connection with geology."

During the later half of the third period, greater progress was made, and before its close geology was thoroughly established as a science. Let us consider for a moment what had really been accomplished up to this time.

It had now been proved beyond question that portions at least of the earth's surface had been covered many times by the sea, with alternations of fresh water and of land, that the strata thus deposited were formed in succession, the lowest of the series being the oldest; that a distinct succession of animals and plants had inhabited the earth during the different geological periods; and that the order of succession found in one part of the earth was essentially the same in all. More than 30,000 new species of extinct animals and plants had now been described. It had been found, too, that from the oldest formations to the most recent, there had been an advance in the grade of life, both animal and vegetable, the oldest forms being among the simplest, and the higher forms successively making their appearance.

It had now become clearly evident, moreover, that the fossils from the older formations were all extinct species, and that only in the most recent deposits were there remains of forms still living. The equally important fact had been established, that in several groups of both animals and plants, the extinct forms were vastly more numerous than the living; while several orders of fossil animals had no representatives in modern times. Human remains had been found mingled with those of extinct animals, but the association was regarded as an accidental one by the authorities in science; and the very recent appearance of man on the earth was not seriously questioned. Another important conclusion reached, mainly through the labors of Lyell, was, that the earth had not been subjected in the past to sudden and violent revolutions; but the changes wrought had been gradual, differing in no respect from those still in progress. Strangely enough, the corollary to this proposition, that life, too, had been continuous on the earth, formed at that date no part of the common stock of knowledge.

In the physical world, the great law of "correlation of forces" had been announced, and widely accepted; but in the organic world, the dogma of the miraculous creation of each separate species still held sway, almost as completely as when Linnæus declared: "There are as many different species as there were different forms created in the beginning by the Infinite Being." But the dawn of a new era was already breaking, and the third period of palæontology we may consider now at an end.

Just twenty years ago, science had reached a point when the belief in "special creations" was undermined by well established facts, slowly accumulated. The time was ripe. Many naturalists were working at the problem, convinced that evolution was the key to the present and the past. But how had nature brought this change about? While others pondered, Darwin spoke the magic word—"*Natural Selection*," and a new epoch in science began.

The fourth period in the history of palæontology dates from this time, and is the period of to-day. One of the main characteristics of this epoch is the belief that *all life, living and extinct, has been evolved from simple forms.* Another prominent feature is the accepted fact of *the great antiquity of the human race.* These are quite sufficient to distinguish this period sharply from those that preceded it.

The publication of Charles Darwin's work on the "Origin of Species," November, 1859, at once aroused attention, and started a revolution which has already in the short space of two decades changed the whole course of scientific thought. The theory of "Natural Selection," or, as Spencer has happily termed it, the "Survival of the Fittest," had been worked out independently by Wallace, who justly shares the honor of the discovery. We have seen that the theory of evolution was proposed and advocated by Lamarck, but he was before his time. The anonymous author of the "Vestiges of Creation," which appeared in 1844, advocated a somewhat similar theory which attracted much attention, but the belief that species were immutable was not sensibly affected until Darwin's work appeared.

The difference between Lamarck and Darwin is essentially this: Lamarck proposed the theory of evolution; Darwin changed this into a doctrine, which is now guiding the investigations in all departments of biology. Lamarck failed to realize the importance of time, and the interaction of life on life. Darwin, by combining these influences with those also suggested by Lamarck, has shown *how* the existing forms on the earth may have been derived from those of the past.

* "Essai géognostique sur le Gisement des Roches," p. 41.

This revolution has influenced palæontology as extensively as any other department of science, and hence the new period we are discussing. In the last epoch, species were represented independently, by parallel lines; in the present period, they are indicated by dependent, branching lines. The former was the analytic, the latter is the synthetic, period. To-day, the animals and plants now living are believed to be genetically connected with those of the distant past; and the palæontologist no longer deems species of the first importance, but seeks for relationships and genealogies, connecting the distant past with the present. Working in this spirit, and with such a method, the advance during the last decade has been great, and is an earnest of what is yet to come.

The progress of palæontology in Great Britain during the present period has been great, and the general interest in the science much extended. The views of Darwin soon found acceptance here. Next to his discovery of "natural selection," Darwin was fortunate in having so able and bold an expounder as Huxley, who was one of the first to adopt his theory, and give it a vigorous support. Huxley's masterly researches have been of great benefit to all departments of biology, and his contributions to palæontology are invaluable. Among the latter, his original investigations on the relations of birds and reptiles are especially noteworthy. His various memoirs on extinct reptiles, amphibians, and fishes, belong to the permanent literature of the subject. The important researches of Owen on the fossil vertebrates have been continued to the present time. He has added largely to his previous publications on the British fossil reptiles, birds, and mammals; the extinct reptiles of South Africa, and the Post-Tertiary birds of New Zealand. His description of the *Archæopteryx*, near the beginning of the period, was a most welcome contribution.

The investigations of Egerton on fossil fishes have likewise been continued with important results. Busk, Dawkins, Flower, and Sanford have made valuable contributions to the history of fossil mammals. Bell, Günther, Hulke, Lankester, Powrie, Miall, and Seely have made notable additions to our knowledge of reptiles, amphibians, and fishes. Among invertebrates, the Crustacea have been especially studied by Jones, Salter, and Woodward. Davidson, Etheridge, Lycett, Morris, Phillips, Wood, and Wright have continued their researches on Mollusks; Duncan, Nicholson, and others have investigated the extinct Corals; and Binney and Caruthers the Fossil Plants. Numerous other important contributions have been made in Great Britain to the science during the present period.

On the Continent, the advance in palæontology has, during the last two decades, been equally great. In France, Gervais continued his memoirs on extinct vertebrates nearly to the present date; while Gaudry has published several volumes on the subject that are models for all students of the science. His work on the fossil animals of Greece is a perfect monograph of its kind, and his later publications are all of importance. Lartet's various works are of permanent value, and his application of palæontology to archæology brought notable results. The volume of Alphonse Milne-Edwards on fossil crustacea was a fit supplement to Brongniart and Desmarets's well known work; while his grand memoir on fossil birds deserves to rank with the classic volumes of Cuvier. Duvernoy, Filhol, Hébert, Sauvage, and others have also published interesting results on fossil vertebrates.

Van Beneden's researches on the fossil vertebrates of Belgium have produced results of great value. Pictet, Rütimeyer, and Wiedersheim in Switzerland, Bianconi, Forsyth-Major and Sismonda in Italy, and Nodot in Spain, have likewise published important memoirs. The extinct vertebrates have been studied in Germany by Von Meyer, Carus, Fraas, Giebel, Haeckel, Haase, Hensel, Kayser, Kner, Ludwig, Peters, Portis, Maack, Salenka, Zittel, and many others; in Denmark by Reinhardt; and in Russia by Brandt and Kowalevsky.

The fossil invertebrates have been investigated with care by D'Archiac, D'Orbigny, Bayle, Fromentel, Oustalet, and others in France; Desor, Loriol, and Roux in Switzerland; Cappellini, Massalongo, Michellotti, Meneghini, and Sismonda in Italy; Barrande, Benecke, Beyrich, Dames, Dorn, Ehlers, Geinitz, Giebel, Gümbel, Feistmantel, Hagen, Von Hauer, Von Heyden, Von Fritsch, Laube, Oppel, Quenstedt, Roemer, Schlüter, Suess, Speyer, and Zittel in Germany, and Winkler in Holland. The fossil plants have been studied in these countries by Massalongo, Saporta, Zigno, Fiedler, Goldenberg, Gehler, Heer, Goeppert, Ludwig, Schimper, Schenk, and many others.

Among the recent researches in palæontology in other regions may be mentioned those of Blanford, Feistmantel, Lydekker, and Stoliczka, in India; Haast and Hector in New Zealand, and Krefft and McCoy in Australia; all of whom have published valuable results.

Of the progress of palæontology in America I have thus far said nothing, and I need now say but little, as many of you are doubtless familiar with its main features. During the first and second periods in the history of palæontology, as I have defined them, America, for most excellent reasons, took no part. In the present century, during the third period, appear the names of Bigsby, Green, Morton, Mitchell, Rafinesque, Say, and Troost, all of whom deserve mention. More recently, the researches of Conrad, Dana, Deane, DeKay, Emmons, Gibbes, Hitchcock, Holmes, Lea, Owen, Redfield, Rogers, Shumard, Swallow, and many others, have enlarged our knowledge of the fossils of this country.

The contributions of James Hall to the invertebrate palæontology of this country form the basis of our present knowledge of the subject. The extensive labors of Meek in the same department are likewise entitled to great credit, and will form an important chapter in the history of the science. The memoirs of Billings, Gabb, Scudder, White, and Whitfield are numerous and important; and the publications of Derby, Hartt, James, Miller, Shaler, Rathburn, and Winchell, are also of value. To Dawson, Lesquereux, and Newberry, we mainly owe our present knowledge of the fossil plants of this country.

The foundation of our vertebrate palæontology was laid by Leidy, whose contributions have enriched nearly every department of the subject. The numerous publications of Cope are well known. Agassiz, Allen, Baird, Dawson, Deane, DeKay, Emmons, Gibbes, Harlan, Hitchcock, Jefferson, Lea, LeConte, Newberry, Redfield, St. John, Warren, Whitney, Worthen, Wyman, and others, have all added to our knowledge of American fossil vertebrates. The chief results in this department of our subject, I have already laid before you on a previous occasion, and hence need not dwell upon them here.

In this rapid sketch of the history of palæontology, I have thought it best to speak of the earlier periods more in detail, as they are less generally known, and especially as they indicate the growth of the science, and the obstacles it had to surmount. With the present work in palæontology, moreover, you are all more or less familiar, as the results are now part of the current literature. To assign every important discovery to its author, would have led me far beyond my present plan. I have only endeavored to indicate the growth of the science by citing the more prominent works that mark its progress, or illustrate the prevailing opinions and state of knowledge at the time they were written.

In considering what has been accomplished, directly or indirectly, it is well to bear in mind that without palæontology there would have been no science of geology. The latter science originated from the study of fossils, and not the reverse, as generally supposed. Palæontology, therefore, is not a mere branch of geology, but the foundation on which that science mainly rests. This fact is a sufficient excuse, if one were wanting, for noting the early opinions in regard to the changes of the earth's surface, as these changes were first studied to explain the position of fossils. The investigation of the latter first led to theories of the earth's formation, and thus to geology. When speculation replaced observation, fossils were discarded, and for a time after this, geologists, as we have seen, apologized for using fossils to determine formations, but for the last half century their value for this purpose has been fully recognized.

The services which palæontology has rendered to botany and zoology are less easy to estimate, but are very extensive. The classification of these sciences has been rendered much more complete by the intercalation of many intermediate forms. The probable origin of various living species has been indicated by the genealogies suggested by extinct types; while our knowledge of the geographical distribution of animals and plants at the present day has been greatly improved by the facts brought out in regard to the former distribution of life on the globe.

Among the vast number of new species which have been added are the representatives of a number of new orders entirely unknown among living forms. The distribution of these extinct orders among the different classes is interesting, as they are mainly confined to the higher groups. Among the fossil plants, no new orders have yet been found. There are none known among the Protozoa or the Mollusca. The Radiates have been enriched by the extinct orders of Blastoidea, Cystidea, and Edrioasterida; and the Crustaceans by the Eurypterida and Trilobita. Among the Vertebrates, no extinct order of fossil fishes has yet been found; but the Amphibians have been enlarged by the important order Labyrinthodonta. The greatest additions have been among the Reptiles, where the majority of the orders are extinct. Here we have at the present date the Ichthyosauria, Sauranodontia, Plesiosauria, and Mosasauria, among the marine forms; the Pterosauria, including the Pteranodontia, containing the flying forms; and the Dinosauria, including the Sauropoda—the giants among reptiles; likewise the Dicynodontia, and probably the Theriodontia, among the terrestrial forms. Although but few fossil birds have been found below the Tertiary, we have already among the Mesozoic forms three new orders: the Saururæ, represented by *Archæopteryx*; the Odontotormæ, with *Ichthyornis* as the type; and the Odontolcæ, based upon *Hesperornis*; all of these orders being included in the sub-class Odontornithes, or toothed birds. Among Mammals, the new groups regarded as orders are the Toxodontia, and the Dinocerata, among the Ungulates; and the Tillodontia, including strange Eocene Mammals whose exact affinities are yet to be determined.

Among the important results in vertebrate palæontology, are the genealogies, made out with considerable probability, for various existing animals. Many of the larger mammals have been traced back through allied forms in a closely connected series to early Tertiary times. In several cases the series are so complete that there can be little doubt that the line of descent has been established. The evolution of the horse, for example, is to-day demonstrated by the specimens now known. The demonstration in one case stands for all. The evidence in favor of the genealogy of the horse now rests on the same foundation as the proof that any fossil bone once formed part of the skeleton of a living animal. A special creation of a single bone is as probable as the special creation of a single species. The method of the palæontologist in the investigation of the one, is the method for the other. The only choice lies between natural derivation and supernatural creation.

For such reasons it is now regarded among the active workers in science as a waste of time to discuss the truth of evolution. The battle on this point has been fought, and won.

The geographical distribution of animals and plants, as well as their migrations, has received much new light from palæontology. The fossils found in some natural divisions of the earth are related so closely to the forms now living there, that a genetic connection between them can hardly be doubted. The extinct Marsupials of Australia, and the Edentates of South America, are well known examples. The Pliocene hippopotami of Asia and the South of Europe point directly to migrations from Africa. Other similar examples are numerous. The fossil plants of the Arctic region prove the existence of a climate there far milder than at present, and recent researches at least render more probable the suggestion, made long ago by Buffon, in his "Epochs of Nature," that life began in the polar regions, and by successive migrations from them the continents were peopled.

The great services which comparative anatomy rendered to palæontology at the hands of Cuvier, Agassiz, Owen, and others, have been amply repaid. The solution of some of the most difficult problems in anatomy has received scarcely less aid from the extinct forms discovered, than from embryology; and the two lines of research supplement each other. Our present knowledge of the vertebrate skull, the limb-arches, and the limbs has been much enlarged by researches in palæontology. On the other hand, the recent labors of Gegenbaur, Huxley, Parker, Balfour, and Thacher, will make clear many obscure points in ancient life.

One of the important results of recent palæontological research is the law of brain-growth, found to exist among extinct mammals, and to some extent in other vertebrates. According to this law, as I have briefly stated it elsewhere: "All Tertiary mammals had small brains. There was, also, a gradual increase in the size of the brain during this period. This increase was confined mainly to the cerebral hemispheres, or higher portions of the brain. In some groups, the convolutions of the brain have gradually become more complicated. In some, the cerebellum and the olfactory lobes have even diminished in size." More recent researches render it probable that the same general law of brain-growth holds good for birds and reptiles from the Mesozoic to the present time. The Cretaceous birds, that have been investigated with reference to this point, had brains only about one-third as large in proportion as those nearest allied among living species. The Dinosaurs from our Western Jurassic follow the same law, and had brain cavities vastly smaller than any existing reptiles. Many other facts point in the same direction, and indicate that the general law will hold good for all extinct vertebrates.

Palæontology has rendered great service to the more recent science of archæology. At the beginning of the present period, a re-examination of the evidence in regard to the antiquity of the human race was going on, and important results were soon attained. Evidence in favor of the presence of man on the earth at a period far earlier than the accepted chronology of six thousand years would imply, had been gradually accumulating, but had been rejected from time to time by the highest authorities. In 1823, Cuvier, Brongniart, and Buckland, and later, Lyell, refused to admit that human relics, and the bones of extinct animals found with them, were of the same geological age, although experienced geologists, such as Boué and others, had been convinced by collecting them. Christol, Serres, and Tournal, in France, and Schmerling in Belgium, had found human remains in caves, associated closely with those of various extinct mammals, and other similar facts were on record.

Boucher de Perthes, in 1841, began to collect stone implements in the gravels of the valley of the Somme, and in 1847, published the first volume of his "Antiquités Celtiques." In this work, he described the specimens he had found and asserted their great antiquity. The facts as presented, however, were not generally accepted. Twelve years later, Falconer, Evans, and Prestwich examined the same localities with care, became convinced, and the results were published in 1859 and 1860. About the same time Gaudry, Hébert, and Desnoyers also explored the same valley, and announced that the stone implements there were as ancient as the mammoth and rhinoceros found with them. Explorations in the Swiss lakes and in the Danish shell heaps added new testimony bearing in the same direction. In 1863 appeared Lyell's work on the "Geological Evidences of the Antiquity of Man," in which facts were brought together from various parts of the world, proving beyond question the great age of the human race.

The additional proof since brought to light has been extensive, and is still rapidly increasing. The Quaternary age of man is now generally accepted. Attempts have recently been made to approximate in years the time of man's first appearance on the earth. One high authority has estimated the antiquity of man merely to the last glacial epoch of Europe as 250,000 years; and those best qualified to judge, would, I think, regard this as a fair estimate.

Important evidence has likewise been adduced of man's existence in the Tertiary, both in Europe and America. The evidence to-day is in favor of the presence of man in the Pliocene of this country. The proof offered on this point by Professor J. D. Whitney, in his recent work,* is so strong, and his careful, conscientious method of investigation so well known, that his conclusions seem inevitable. Whether the Pliocene strata he has explored so fully on the Pacific coast corresponds strictly with the deposits which bear this name in Europe, may be a question requiring further consideration. At present, the known facts indicate that the American beds containing human remains, and works of man, are at least as old as the Pliocene of Europe. The existence of man in the Tertiary period seems now fairly established.

In looking back over the history of palæontology, much seems to have been accomplished; and yet the work has but just begun. A small fraction only of the earth's surface has been examined, and two large continents are waiting to be explored. The "imperfection of the geological record," so often cited by friends and foes, still remains, although much improved; but the future is full of promise. In filling out this record, America, I believe, will do her full share, and thus aid in the solution of the great problems now before us.

I have endeavored to define clearly the different periods in the history of palæontology. If I may venture, in conclusion, to characterize the present period in all departments of science, its main feature would be a *belief in universal laws*. The reign of Law, first recognized in the physical world, has now been extended to Life as well. In return, Life has given to inanimate nature the key to her profounder mysteries—evolution, which embraces the universe.

What is to be the main characteristic of the next period? No one now can tell. But if we are permitted to continue in imagination the rapidly converging lines of research pursued to-day, they seem to meet at the point where organic and inorganic nature become one. That this point will yet be reached, I cannot doubt.

[Continued from SUPPLEMENT No. 193.]

MYTHOLOGIC PHILOSOPHY.

Vice-Presidential Address of Prof. J. W. POWELL, of Washington, Vice-President Section B, American Association for the Advancement of Science, Saratoga Meeting, August, 1879.

OUTGROWTHS FROM MYTHOLOGIC PHILOSOPHY.

THE three stages of mythologic philosophy that are still extant in the world must be more thoroughly characterized, and the course of their evolution indicated. But in order to do this clearly, certain outgrowths from mythologic philosophy must be explained, certain theories and practices that necessarily result from this philosophy, and that are intricately woven into the institutions of mankind.

ANCIENTISM.

The first I denominate ancientism. Yesterday was better than to-day. The ancients were wiser than we.

This belief in a better day and a better people in the older time is almost universal among mankind.

A belief so widely spread, so profoundly entertained, must have for its origin some important facts in the constitution or history of mankind. Let us see what they are. In the history of every individual, the sports and joys of childhood are compared and contrasted with the toils and pains of old age. Greatly protracted life, in savagery and barbarism, is not a boon to be craved.

In that stage of society where the days and the years go by with little or no provision for a time other than that which is passing, the old must go down to the grave through poverty and suffering. In that stage of culture, to-morrow's bread is not certain, and to-day's bread is often scarce. In civilization, plenty and poverty live side by side; the palace

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* "Auriferous Gravels of the Sierra Nevada of California." 1879.

and the hovel are on the same landscape; the rich and poor elbow each other on the same street; but, in savagery, plenty and poverty come with recurring days to the same man, and the tribe is rich to-day and poor to-morrow, and the days of want come in every man's history, and when they come the old suffer most, and the burden of old age is oppressive. In youth, activity is joy; in old age, activity is pain. No wonder, then, that old age loves youth, or that to-day loves yesterday, for the instinct is born of the inherited experiences of mankind.

But there is yet another and more potent reason for ancientism. That tale is the most wonderful that has been most repeated, for the breath of speech is the fertilizer of story. Hence, the older the story, the greater its thaumaturgics. Thus, yesterday is greater than to-day by natural processes of human exaggeration.

Again, that is held to be most certain, and hence most sacred, which has been most often affirmed.

A Brahman was carrying a goat to the altar. Three thieves would steal it. So they placed themselves at intervals along the way by which the pious Brahman would travel. When the venerable man came to the first thief he was accosted: "Brahman, why do you carry a dog?" Now, a dog is an unclean beast, and no Brahman must touch it. And the Brahman, after looking at his goat, said: "You do err; this is a goat." And when the old man reached the second thief, again he was accosted: "Brahman, why do you carry a dog?" So the Brahman put his goat on the ground, and, after narrowly scrutinizing it, he said: "Surely, this is a goat," and went on his way. When he came to the third thief he was once more accosted: "Brahman, why do you carry a dog?" Then the Brahman, having thrice heard that his goat was a dog, was convinced, and throwing it down, he fled to the temple for ablution, and the thieves had a feast.

The child learns not for himself, but is taught, and accepts as true that which is told, and a propensity to believe the affirmed is implanted in his mind.

In every society some are wise and some are foolish, and the wise are revered, and their affirmations are accepted. Thus, the few lead the multitude in knowledge, and the propensity to believe the affirmed started in childhood, is increased in manhood in the great average of persons constituting society, and these propensities are inherited from generation to generation, until we have a cumulation of effects.

The propagation of opinions by affirmation, the cultivation of the propensity to believe that which has been affirmed many times, let us call *affirmatization*.

If the world's opinions were governed only by the principles of mythologic philosophy, affirmatization would become so powerful that nothing would be believed but the affirmed. Men would come to no new knowledge.

Society would stand still, listening to the wisdom of the fathers. Such a condition of affairs actually existed for centuries in China. But the power of affirmatization is steadily undermined by science.

And, still again, the institutions of society conform to its philosophy.

The explanation of things always include the origin of human institutions. So the welfare of society is based on philosophy, and the venerable sayings which constitute philosophy are thus held as sacred.

So ancientism is developed from accumulated life experiences; by the growth of story in repeated narration; by the steadily increasing power of affirmatization, and by respect for the authority upon which the institutions of society are based; all accumulating as they come down the generations. That we do thus inherit effects we know, for has it not been affirmed in the Book that the "fathers have eaten grapes, and the children's teeth are set on edge"?

As men come to believe that the "long ago" was better than the "now," and the dead were better than the living, then philosophy must necessarily include a theory of degeneracy which is a part of ancientism.

THEISTIC SOCIETY.

Again, the actors in mythologic philosophy are personages, and we always find them organized in societies.

The social organization of mythology is always found to be essentially identical with the social organization of the people who entertain the philosophy. The gods are husbands and wives, and parents and children, and the gods have an organized government. This gives us theistic society, and we cannot properly characterize a theism without taking its mythic society into consideration.

SPIRITISM.

In the earliest stages of society of which we have practical knowledge by acquaintance with the people themselves, a belief in the existence of spirits prevails—a shade, an immaterial existence, which is the duplicate of the material personage. The genesis of this belief is complex. The workings of the human mind during periods of unconsciousness lead to opinions that are enforced by many physical phenomena.

First, we have the activities of the mind during sleep, when the man seems to go out from himself to converse with his friends, to witness strange scenes, and to have many wonderful experiences. Thus, the man seems to have lived an eventful life, when his body was, in fact, quiescent and unconscious. Memories of scenes and activities in former days, and the inherited memories of scenes witnessed and actions performed by ancestors, are blended in strange confusion by broken and inverted sequences. Now and then the dream scenes are enacted in real life, and the infrequent coincidence, or apparent verification, makes deep impression on the mind, while unfulfilled dreams are forgotten. Thus the dreams of sleepers are attributed to their immaterial duplicates, their spirits.

In many diseases, also, the mind seems to wander to see sights, and to hear sounds, and to have many wonderful experiences, while the body itself is apparently unconscious. Sometimes on restored health, the person may recall these wonderful experiences, and during their occurrence the subject talks to unseen persons, and seems to have replies, and to act, to those who witness, in such a manner that a second self—a spirit independent of the body—is suggested. When disease amounts to long-continued insanity, all of these effects are greatly exaggerated, and make a deep impression upon all who witness the phenomena.

Thus the hallucinations of fever-racked brains and mad minds are attributable to spirits.

The same conditions of apparent severance of mind and body witnessed in dreams and hallucinations are often produced artificially in the practice of *ecstasism*.

In the vicissitudes of savage life, while little or no provision is made for the future, there are times when the savage resorts to almost anything at hand as a means of subsistence, and thus all plants, and all parts of plants, seed, fruit, flowers, leaves, bark, roots—anything in times of extreme want—may be used as food. But experience soon teaches the various effects upon the human system which is produced by the several vegetable substances with which he meets, and thus the effect of narcotics is early discovered, and the savage, in the practice of his religion, ofttimes resorts to these native drugs for the purpose of producing an ecstatic state under which divination may be performed. The practice of ecstasism is universal in the lower stages of culture. In times of great anxiety every savage and barbarian seeks to know of the future.

Through all the earlier generations of mankind ecstasism has been practiced, and civilized man has thus an inherited appetite for narcotics, to which the enormous propensity to drunkenness existing in all nations bears witness.

When the great actor in his personation of Rip Van Winkle holds his goblet aloft and says, "Here's to your health, and to your family's, and may they live long and prosper," he connects the act of drinking with a prayer, and unconsciously demonstrates the origin of the use of stimulants. It may be, that when the jolly companion has become a loathsome sot, and his mind is ablaze with the fire of drink, and he sees uncouth beasts in horrid presence, inherited memories haunt him with visions of the beast gods worshiped by his ancestors at the very time when the appetite for stimulants was created. But ecstasism is produced in other ways, and for this purpose the savage or barbarian often resorts to fasting and bodily torture.

In many ways he produces the wonderful state, and the visions of ecstasy are interpreted as the evidence of spirits.

Many physical phenomena serve to confirm this opinion. It is very late in philosophy when shadows are referred to the interception of the rays of the sun.

In savagery and barbarism shadows are supposed to be emanations from or duplicates of the bodies causing the shadows. And what savage understands the reflection of the rays of the sun by which images are produced?

They also are supposed to be emanations or duplications of the object reflected. No savage or barbarian could understand that the waves of the air are turned back, and sound is duplicated in an echo. He knows not that there is an atmosphere, and to him the echo is the voice of an unseen personage—a spirit. There is no theory more profoundly implanted in early mankind than that of spiritism.

THAUMATURGICS.

The gods of mythologic philosophies are created to account for the wonders of nature. Necessarily they are a wonder-working folk, and having been endowed with these magical powers in all the histories given in mythic tales of their doings on the earth, we find them performing most wonderful feats.

They can transform themselves, they can disappear and reappear, all their senses are magical, some are endowed with a multiplicity of eyes, others have a multiplicity of ears; in Norse mythology the watchman on the rainbow bridge could hear the grass grow and the wool on the backs of sheep; arms can stretch out to grasp the distance, tails can coil about mountains, and all powers become magical.

But the most wonderful power with which the gods are endowed is the power of will, for we find that they can think their arrows to the hearts of their enemies, mountains are overthrown by thought, and thoughts are projected into other minds. Such are the thaumaturgics of mythologic philosophy.

MYTHIC TALES.

Early man having created through the development of his philosophy a host of personages, these gods must have a history.

A part of that history, and the most important part to us as students of philosophy, is created in the very act of creating the gods themselves. I mean that portion of their history which relates to the operations of nature, for the gods were created to account for those things. But to this is added much else of adventure. The gods love as men love, and go in quest of mates; the gods hate as men hate, and fight in single combat or engage in mythic battles; and the history of these adventures, impelled by love and hate, and all other passions and purposes with which men are endowed, all woven into a complex tissue with their doings, in carrying out the operations of nature, constitute the web and woof of mythology.

RELIGION.

Again, as human welfare is deeply involved in the operations of nature, man's chief interest is in the gods.

In this interest religion originates. Man, impelled by his own volition, guided by his own purposes, aspires to a greater happiness, and endeavor follows endeavor, but, at every step, his progress is impeded; his own powers fail before the greater powers of nature. His powers are pygmies, nature's powers are giants, and to him these giants are gods with wills and purposes of their own, and he sees that man in his weakness can succeed only by allying himself with the gods. Hence, impelled by this philosophy, man must have communion with the gods, and in this communion he must influence them to work for himself. Hence, religion, which has to do with the relations which exist between the gods and man, is the legitimate offspring of mythologic philosophy. Thus we see that out of mythologic philosophy, as branches of the great tree itself, there grow ancientism, theistic society, spiritism, thaumaturgics, mythic tales, and religion.

THE EVOLUTION OF MYTHOLOGIC PHILOSOPHY.

I shall now give a summary characterization of zootheism; then call attention to some of the relics of hecastotheism found therein, and proceed with a brief statement of the higher stages of theism. The apparent and easily accessible is studied first. In botany, the trees and the conspicuous flowering plants of garden, field, and plain were first known, and then all other plants were vaguely grouped as weeds, but since the most conspicuous phenogamous plants were first studied, what vast numbers of new orders, new genera, and new species have been discovered in the progress of research to the lowest cryptogams.

In the study of ethnology, we first recognized the more civilized races. The Aryan, Hamites, Shemites, and Chinese, and the rest were the weeds of humanity—the barbarian, and savage, sometimes called Turanians.

But when we come carefully to study these lower people, what numbers of races are discovered. In North America alone we have more than seventy-five stocks of people speaking seventy-five stocks of language, and some single stocks embracing many distinct languages and dialects.

The languages of the Algonkin family are as diverse as the Indo-European tongues. So are the languages of the Dakota, the Numa, the Tinné, and others; so that in North America we have more than five hundred languages spoken to-day. Each linguistic stock is found to have a philosophy of its own, and each stock as many branches of philosophy as it has languages and dialects.

North America presents a magnificent field for the study of savage and barbaric philosophies.

This vast region of thought has been explored only by a few adventurous travelers in the world of science.

No thorough survey of any part has been made. Yet the general outlines of North American philosophy are known, but the exact positions, the details, are all yet to be filled in as the geography of the general outline of North America is known by exploration, but the exact positions and details of topography are yet to be filled in as the result of careful survey. Myths of the Algonkin stock are found in many a volume of *Americana*, the best of which were recorded by the early missionaries who came from Europe, though we find some of them, mixed with turbid speculations, in the writings of Schoolcraft. Many of the myths of the Indians of the South, in that region stretching back from the great Gulf, are known, some collected by travelers, others by educated Indians.

Many of the myths of the Iroquois are known. The best of these are in the writings of Morgan, America's greatest anthropologist. Missionaries, travelers, and linguists have given us a great store of the myths of the Dakota stock. Many myths of the Tinné, also, have been collected. Petitot has recorded a number of those found at the North, and we have in manuscript some of the myths of a southern branch—the Navahoes.

Perhaps the myths of the Numas have been collected more thoroughly than those of any other stock. These are yet unpublished. Powers has recorded many of the myths of various stocks in California, and the old Spanish writings give us a fair collection of the Nahuatl myths of Mexico, and Rink has presented us an interesting volume on the mythology of the Innuits, and, finally, fragments of mythology have been collected from nearly all the tribes of North America, and they are scattered through thousands of volumes, so that the literature is vast. The brief description which I shall give of zootheism is founded on a study of the materials which I have thus indicated.

All these tribes are found in the higher stages of savagery or the lower stages of barbarism, and their mythologies are found to be zootheistic among the lowest, physitheistic among the highest, and a great number of tribes are found in a transition state, for zootheism is found to be a characteristic of savagery, and physitheism of barbarism, using the terms as they have been defined by Morgan. The supreme gods of this stage are animals.

The savage is intimately associated with animals.

"From them he obtains the larger part of his clothing, and much of his food, and he carefully studies their habits, and finds many wonderful things. Their knowledge, and skill, and power appear to him to be superior to his own. He sees the mountain sheep fleet among the crags, the eagle soaring in the heavens, the humming-bird poised over its blossom cup of nectar, the serpent swift without legs, the salmon scaling the rapids, the spider weaving its gossamer web, the ant building a play-house mountain—in all animal nature he sees things too wonderful for him, and from admiration he grows to adoration, and the animals become his gods." Ancientism plays an important part in this zootheism. It is not the animals of to-day whom the Indians worship, but their progenitors—their prototypes. The wolf of to-day is a howling pest, but that wolf's ancestor—the first of the line—was a god. The individuals of every species are supposed to have descended from an ancient being—a progenitor of the race—and so they have a grizzly-bear-god, an eagle-god, a rattlesnake-god, a trout-god, a spider-god—a god for every species and variety of animals.

By these animal gods all things were established. The heavenly bodies were created and their ways appointed, and when the powers and phenomena of nature are personified, the personages are beasts, and all human institutions also were established by ancient animal gods.*

The ancient animals of any philosophy of this stage are found to constitute a clan or gens—a body of relatives, or consanguinii, with grandfathers, fathers, sons, and brothers.

In Ute theism, the ancient Togoav, the first rattlesnake, is the grandfather, and all the animal gods are assigned to their relationships.

Grandfather Togoav, the wise, was the chief of the council, but Shinauav, the ancient wolf, was the chief of the clan.

There were many other clans and tribes of ancient gods with whom these supreme gods had dealings, of which, hereafter and finally, each of these ancient gods became the progenitor of a new tribe, so that we have a tribe of bears, a tribe of eagles, a tribe of rattlesnakes, a tribe of spiders, and many other tribes, as we have tribes of Utes, tribes of Sioux, tribes of Navahoes; and, in that philosophy, tribes of animals are considered to be co-ordinate with tribes of men.

All of these gods have invisible duplicates—spirits—and they have often visited the earth.

All of the wonderful things seen in nature are done by the animal gods. That elder life was a magic life; but the descendants of the gods are degenerate.

Now and then as a medicine man by practicing sorcery can perform great feats, so now and then there is a medicine bear, a medicine wolf, or a medicine snake that can work magic.

On winter nights the Indians gather about the camp fire, and then the doings of the gods are recounted in many a mythic tale. I have heard the venerable and impassioned orator on the camp meeting stand rehearse the story of the crucifixion, and have seen the thousands gathered there weep in contemplation of the story of divine suffering, and heard their shouts roll down the forest aisles as they gave vent to their joy at the contemplation of redemption. But the scene was not a whit more dramatic than another I have witnessed in an evergreen forest of the Rocky Mountain region, where a tribe was gathered under the great pines, and the temple of light from the blazing fire was walled by the darkness of midnight, and in the midst of the temple stood the wise old man telling in simple savage language the story of Tawats when he conquered the sun and established the seasons and the days.

In that pre-Columbian time, before the advent of white men, all the Indian tribes of North America gathered on winter nights by the shores of the seas, where the tides beat in solemn rhythm, by the shores of the great lakes, where the waves dashed against frozen beaches, and by the banks of the rivers flowing ever in solemn mystery—each in its own temple of illumined space—and listened to the story of its own supreme gods—the ancients of time.

* Vide "Outlines of the Philosophy of the North American Indians, by J. W. Powell," read before the American Geographical Society, at Chickering Hall, December 29, 1876.

Religion, in this stage of theism, is sorcery. Incantation, dancing, fasting, bodily torture, and ecstasism are practiced. Every tribe has its potion or vegetable drug, by which the ecstatic state is produced, and their venerable medicine men see visions and dream dreams. No enterprise is undertaken without consulting the gods, and no evil impends but they seek to propitiate the gods. All daily life, to the minutest particular, is religious. This stage of religion is characterized by fetishism. Every Indian is provided with his charm or fetish, revealed to him in some awful hour of ecstasy, produced by fasting or feasting, or drunkenness, and that fetish he carries with him to bring good luck in love or in combat, in the hunt or on the journey. He carries a fetish suspended to his neck; he ties a fetish to his bow; he buries a fetish under his tent; he places a fetish under his pillow of wild-cat skins; he prays to his fetish; he praises it or chides it; if successful, his fetish receives the glory; if he fail, his fetish is disgraced. These fetishes may be fragments of bone or shell, the tips of the tails of animals, the claws of birds or beasts, perhaps dried hearts of little warblers, shards of beetles, leaves powdered and held in bags, or crystals from the rocks—anything curious may become a fetish. Fetishism, then, is a religious means, not a philosophic or mythologic state.

Such are the supreme gods of the savage, and such the institutions which belong to their theism.

But they have many other inferior gods. Mountains, hills, valleys, and great rocks have their own special deities—invisible spirits—and lakes, rivers, and springs are the homes of spirits. But all these have animal forms when in proper *personæ*. Yet some of the medicine spirits can transform themselves and work magic as do medicine men. The heavenly bodies are either created personages or ancient men or animals translated to the sky. And last, we find that ancestors are worshiped as gods. Among all the tribes of North America with which we are acquainted tutelarism prevails.

Every tribe and every clan has its own protecting god, and every individual has his "my god." It is a curious fact that every Indian seeks to conceal the knowledge of his "my god" from all other persons, for he fears that if his enemy should know of his tutelar deity he might, by extraordinary magic, succeed in estranging him, and be able to compass his destruction through his own god.

In this summary characterization of zootheism, I have necessarily systematized my statements. This, of course, could not be done by the savage himself. He could give you its particulars, but could not group those particulars in any logical way. He does not recognize any system, but talks indiscriminately, now of one, now of another god, and with him the whole theory as a system is vague and shadowy; but its particulars are vividly before his mind, and the certainty with which he entertains his opinions leaves no room to doubt his sincerity.

But there is yet another phase of theism discovered. Sometimes a particular mountain or hill, or some great rock, some waterfall, some lake, or some spring receives special worship, and is itself believed to be a deity. This seems to be a relic of hecastotheism.

Fetishism also seems to have come from that lower grade, and all the minor deities, the spirits of mountains and hills and forests, seem to have been derived from that same stage, but with this development, that the things themselves are not worshiped, but their essential spirits.

From zootheism, as described, to physitheism the way is long. Gradually, in the progress of philosophy, animal gods are dethroned and become inferior diamons or are forgotten, and gradually the gods of the firmament—the sun, the moon, the stars—are advanced to supremacy; the clouds, the storms, the winds, day and night, dawn and gloaming, the sky, the earth, the sea, and all the various phases of nature perceived by the barbaric mind, are personified and deified, and exalted to a supremacy co-ordinate with the firmament gods, and all the gods of the lower stage that remain—animals, daimons, and all men—belong to inferior tribes. The gods of the sky—the shining ones, those that soar on bright wings, those that are clothed in gorgeous colors, those that came from—we know not where, those that vanish to the unknown—are the supreme gods.

We always find these gods organized in great tribes, with mighty chieftains, who fight in great combats or lead their hosts in battle and return with much booty. Such is the theism of ancient Mexico; such the theism of the Northland, and such the theism discovered among the ancient Aryans.

From this stage to psychotheism the way is long, for evolution is slow. Gradually men come to differentiate more carefully between good and evil, and the ethic character of their gods becomes the subjects of consideration, and the good gods grow in virtue, and the bad gods grow in vice. Their identity with physical objects and phenomena is gradually lost. The different phases or conditions of the same object or phenomena are severed, and each is personified. The bad gods are banished to underground homes, or live in concealment, from which they issue on their expeditions of evil. Yet, still, all powers exist in these gods, and all things were established by them.

With the growth of their moral qualities no physical powers are lost, and the spirits of the physical bodies and phenomena become daimons, subordinate to the great gods who preside over nature and human institutions.

We find, also, that these superior gods are organized in societies. I have said the Norse mythology was in a transition state from physitheism to psychotheism. The Asas, or gods, lived in Asgard, a mythic communal village, with its thing or council, the very counterpart of the communal village of Iceland. Olympus was a Greek city. Still further in the study of mythologic philosophy we see that more, and supremacy falls into the hands of the few, until monotheism is established on the plan of the empire.

Then all of the inferior deities whose characters are pure become ministering angels, and the inferior deities whose characters are evil become devils, and the differentiation of good and evil is perfected in the gulf between heaven and hell.

In all this time from zootheism to monotheism, ancientism becomes more ancient, and the times and dynasties are multiplied. Spiritism is more clearly defined, and spirits become eternal; mythologic tales are codified; divination for the result of amorous intrigue has become the prophecy of immortality, and sacred books are written, and thaumaturgics is formulated as the omnipotent, the omnipresent, and the infinite.

Time has failed me to tell of the evolution of idolatry from fetishism, priestcraft from sorcery, and of their overthrow by the doctrines that were uttered by that Voice on the Mount.

Religion, that was fetishism and ecstasism and sorcery, is now the yearning for something better, something purer, and the means by which this highest state for humanity may be reached, the ideal worship of the highest monotheism, is "in spirit and in truth." The steps are long from Shinauav, the ancient of wolves, by Zeus, the ancient of skies, to Jehovah, "the ancient of days."

Comparative theology furnishes grand illustrations of the processes of evolution. It presents a multiplicity of events occurring in orderly succession in obedience to the laws of adaptation, heredity, and survival of the fittest, and in passing from the lower to the higher state it demonstrates the fundamental law of progress, that evolution is from the homogeneous to the heterogeneous by successive differentiations and integrations. When society shall have passed to complete integration in the unification of the nations, and differentiation is perfected in universal liberty, then the sole philosophy will be science. How long! Oh! how long!

How the attitude of man has changed with the change of his philosophy from mythologic to scientific methods!

Trembling man, palsied with false philosophy, exclaims: "Our Father, who art in heaven, hallowed be Thy name. Thy kingdom come. Thy will be done;" and having thus abdicated his own throne, on his knees a beggar, he prays his God: "Give us this day our daily bread." In scientific philosophy, the phenomena of nature are an orderly succession of events, and move in obedience to law. By a knowledge of law the powers of nature are the slaves of man. And the philosopher of science, from the throne of his reason, stretches out the scepter of his knowledge, and issues his mandates to the subjected powers of nature, "Give us this day our daily bread!" Be ye slaves, or be ye masters?

A SHORT BIOGRAPHY OF THE MENHADEN.

By PROF. G. BROWNE GOODE, U. S. Fish Commissioner.

Read before the American Association, Saratoga Springs, 1879.

THE herring family is represented on the Atlantic coast of the United States by ten species, all of which swim in immense schools, and several, such as the sea-herring, the shad, and the various species of the river alewives, are of great economical importance.

In abundance and value these are all surpassed by the menhaden (*Brevoortia tyrannus*), a fish whose habits are in many respects anomalous, and concerning which very little has been known and written.

The menhaden has at least thirty distinct popular names, most of them limited in their use within narrow geographical boundaries. To this circumstance may be attributed the prevailing ignorance among our fishermen regarding its habits and migrations, which has perhaps prevented its more extensive utilization, particularly in the South.

North of Cape Cod the name "pogy" is almost universally in use, while in Southern New England the fish is known only as "the menhaden." These two names are derived from two Indian words of the same meaning; the first being the Abnaki name "*pookagan*" or "*poghaden*," which means "fertilizer," while the latter is a modification of a word which in the Narragansett dialect meant "that which enriches the earth." About Cape Ann, "pogy" is partially replaced by "hard-head" or "hard-head shad," and in Eastern Connecticut by "bony fish." In Western Connecticut the species is generally known as the "white fish," while in New York the usage of two centuries is in favor of "mossbunker." This name is a relic of the Dutch colony of New Amsterdam, having evidently been transferred from the "scad," or "horse mackerel" (*Caranx trachurus*), a fish which visits the shores of Northern Europe in immense schools, swimming at the surface in much the same manner as our menhaden, and known to the Hollanders as the "marshbanker." New Jersey uses the New York name with its local variations, such as "bunker" and "marshbanker." In Delaware Bay, the Potomac, and the Chesapeake, we meet with the "alewife" and "green tail." Virginia gives us "bug fish," "bug head," and "bug shad," referring to the parasitic crustacean found in the mouths of all Southern menhaden. In North Carolina occurs the name "fat back," which prevails as far south as Florida, and refers to the oiliness of the flesh. In this vicinity, too, the names "yellow-tail" and "yellow-tailed shad" are occasionally heard, while in Southern Florida the fish is called "shiner" and "herring." In South America, among the Portuguese, the name "savega" is in use. On the St. John's River, and wherever Northern fishermen are found, "menhaden" is preferred, and it is to be hoped that this name will in time be generally adopted. A number of trade names are employed by the manufacturers in New Jersey who can this fish for food. These are, "American sardine," "American club fish," "shadine," and "ocean trout."

In 1815 the species was described by Mitchill, of New York, under the name *Clupea menhaden*, which has since been commonly accepted. A prior description by Latrobe, in 1809, long lost sight of, renders it necessary, as I have elsewhere demonstrated, to adopt the specific name *Tyrannus*. The genus *Brevoortia*, of which this species is the type, was established by Gill in 1861.

The geographical range of *Brevoortia tyrannus* varies from year to year. For 1877 it was, so far as it is possible to define it in words, as follows: The wanderings of the species are bounded by the parallels of north latitude 25° and 45°; on the continental side by the line of brackish water; on the east by the inner boundary of the Gulf Stream. In the summer it occurs in the coastal waters of all the Atlantic States from Maine to Florida. In winter, only south of Cape Hatteras. The limits of its winter migration oceanwards cannot be defined, though it is demonstrated that the species does not occur about the Bermudas or Cuba, nor presumably in the Caribbean Sea. In Brazilian waters occurs a geographical race of the same species, *Brevoortia tyrannus*, subspecies *Aurea*, the *Clupanodon aureus* of Agassiz and Spix; on the coast of Paraguay and Patagonia by *Brevoortia pectinata;* in the Gulf of Mexico by *Brevoortia patronus*.

With the advance of spring the schools of menhaden appear near our coasts in company with, and usually slightly in advance of, the other non-resident species, such as the shad, alewives, bluefish, and squeteague. The following general conclusions regarding their movements are deduced from the statements of about two hundred observers at different points on the coasts from Florida to Nova Scotia.

At the approach of settled warm weather they make their appearance in the inshore waters. It is manifestly impracticable to indicate the periods of their movements except in an approximate way. The comparison of two localities distant apart one or two hundred miles will indicate very little. When wider ranges are compared there becomes perceptible a certain proportion in the relations of the general averages. There is always a balance in favor of earlier arrivals in the more southern localities. Thus it becomes apparent that the first schools appear in Chesapeake Bay in March and April; on the coast of New Jersey in April and early May; on the south coast of New England in late April and May; off Cape Ann about the middle of May; and in the Gulf of Man in the latter part of May and the first of June. Returning, they leave Maine in late September and October; Massachusetts in October, November, and December, the latest departures being those of fish which have been detained in the land-locked bays and creeks; Long Island Sound and vicinity in November and December; Chesapeake Bay in December; and Cape Hatteras in January. Further to the south they appear to remain more or less constantly throughout the year.

A strange fact is that their northern range has become considerably restricted within the past 25 years. Perley, writing in 1852, stated that they were sometimes caught in considerable numbers about St. John's, N. B., and there is abundance of other testimony to the fact that they formerly frequented the Bay of Fundy in its lower parts. At present the eastward wanderings of the schools do not extend beyond Isle au Haut and Great Duck Island, about 40 miles west of the boundaries of Maine and New Brunswick. They have not been known to pass these limits for 10 or 15 years. They have this year hardly passed north of Cape Cod, and forty or more steamers which have usually reaped an extensive harvest on the coast of Maine, have been obliged to return to the fishing grounds of Southern New England, where menhaden are found as abundantly as ever.

I have elsewhere shown the arrival of the menhaden schools to be closely synchronous with the period at which the weekly average of the surface temperatures of the harbors rises to 51° F.; that they do not enter waters in which, as about Eastport, Me., the midsummer surface temperatures, as indicated by monthly averages, fall below 51° F., and that their departure in the autumn is closely connected with the fall of the thermometer to 51° and below. In 1877 a cold summer seemed to threaten the success of the Maine menhaden fisheries. In September and October, however, the temperatures were higher than in the corresponding months of the previous year, and the scarcity of the early part of the season was amply amended for. The causes of the capricious movements in 1879 have not been fully made out, but there is reason to believe that they relate to temperature. After the appearance of the vanguard they rapidly increase in abundance until the sea appears to be alive with them. They delight to play in inlets and bays, such as the Chesapeake, Peconic, and Narragansett bays, and the narrow fiords of Maine. They seem particularly fond of shallow waters protected from the wind, in which, if not molested, they will remain throughout the season, drifting in and out with the tide. Brackish water attracts them, and they abound at the mouths of streams, especially on the southern coast. They ascend the St. John's River more than 30 miles, the St. Mary's, the Neuse, the York, the Rappahannock, the Potomac nearly to Washington, and the Pawtuxent to Marlboro. They come in with or before the shad, and are very troublesome to the fishermen by clogging their nets. I am not aware that this difficulty occurs in northern rivers, though they are found in the summer in the Hudson and its tributaries, the Housatonic, Mystic, Thames, and Providence rivers, in the creeks of Cape Cod, and at the mouth of the Merrimac. A curious instance of capriciousness in their movements occurred on the coast of Maine, where much alarm was felt, because their habits were thought to have been changed through the influence of seining. The shore fishermen could obtain none for bait, and vessels followed them far out to sea, capturing them in immense quantities 40 miles from the land. The fisheries had produced no such effect south of Cape Cod, and it was quite inexplicable that their habits should have been so modified in the north. In 1878, however, after ten years or more, they resume their former habits of hugging the shores, and the menhaden fishery of Maine was carried on for the most part in the rivers.

The arrival of the menhaden is announced by their appearance at the top of the water. They swim in immense schools, their heads close to the surface, packed side by side, and often tier above tier, almost as closely as sardines in a box. A gentle ripple indicates their position, and this may be seen at a distance of nearly a mile by the lookout at the masthead of a fishing vessel, and is of great assistance to the seiners in setting their nets. At the slightest alarm the school sinks toward the bottom, often escaping its pursuers. Sailing over a body of menhaden swimming at a short distance below the surface, one may see their glittering backs beneath, and the boat seems to be gliding over a floor inlaid with blocks of silver. At night they are phosphorescent. Their motions seem capricious and without a definite purpose; at times they swim around and around in circles; at other times they sink and rise. Why they remain thus at the surface, so conspicuous a prey to men, birds, and other fishes, is not known. It does not appear to be for the purpose of feeding; perhaps the fisherman is right when he declares that they are playing.

An old mackerel fisherman thus describes the difference in the habits of the mackerel and menhaden: "Pogies school differently from mackerel; the pogy slaps with his tail; and in moderate weather you can hear the sound of a school of them as first one and then another strikes the water. The mackerel go long 'gilling'—that is, putting the sides of their heads out of the water as they swim. The pogies make a flapping sound; the mackerel a rushing sound. Sometimes in calm and foggy weather you can hear a school of mackerel miles away."

They do not attract small birds as do the schools of predaceous fish. The fish-hawk often hovers above them, and some of the larger gulls occasionally follow them in quest of a meal. About Cape Cod one of the gulls, perhaps *Larus argentatus*, is called "pogy gull."

On warm, still, sunny days, the fish may always be seen at the surface, but cold or rainy weather and prevailing northerly or easterly winds quickly cause them to disappear. When it is rough they are not so often seen, though schools of them frequently appear when the sea is too high for fishermen to set their nets. The best days for menhaden fishing are when the wind is north-westerly in the morning, dying out in the middle of the day, and springing up again in the afternoon from the southwest, with a clear sky. At the change of the wind on such a day they come to the surface in large numbers.

A comparison of the influence of the weather upon the menhaden and the herring yields some curious results. The latter is a cold water species. With the advance of summer it seeks the north, returning to our waters with the approach of cold. The menhaden prefers the temperature of 60° or more; the herring, 55° and less. When the menhaden desert the Gulf of Maine they are replaced by the herring. Cold weather drives the former to the warmer strata, while it brings the latter to the surface. The conditions most favorable on our coast for the appearance of herring on the

surface, and which correspond precisely with those which have been made out for the coast of Europe, are least so for the menhaden.

Their winter habitat, like that of the other cold weather absentees, has never been determined. The most plausible hypothesis supposes that instead of migrating toward the tropics or hibernating near the shore, as has been claimed by many, they swim out to sea until they find a stratum of water corresponding to that frequented by them during their summer sojourn on the coast. This is rendered probable by the following considerations: 1. That the number of menhaden in southern waters is neither less in the season of their abundance nor greater in that of their absence from the north coast. 2. That there are local varieties of the species, distinguished by physical characters almost of specific value, by differences in habits, and, in the case of the southern schools, by the universal presence in the mouth of a crustacean parasite, which is never found with those north of Cape May. 3. That the same schools usually reappear in the same waters in successive years. 4. That their very prompt arrival in the spring suggests their presence in waters near at hand. 5. That their leanness when they first appear renders it evident that they have had no food since leaving the coast in autumn; the latter consideration, since they are bottom feeders, is the strongest confirmation of the belief that their winter home is in the mid-oceanic sub-strata.

As is indicated by the testimony of a large number of observers, whose statements are elsewhere reviewed at length, the menhaden is by far the most abundant species on the eastern coast of the United States. Several hundred thousand are frequently taken in a single draught of a purse-seine. A firm in Milford, Connecticut, captured in 1870, 8,000,000; in 1871, 8,000,000; in 1872, 10,000,000; in 1873, 12,000,0 0. In 1877, three sloops from New London seined 13,000,000. In 1877—an uprofitable year—the Pemaquid Oil Company took 20,000,000, and the town of Boothbay alone 50,000,000. There is no evidence whatever of any decrease in their numbers, though there can be in the nature of the case absolutely no data for comparison of their abundance in successive years. Since spawning menhaden are never taken in the nets, no one can reasonably predict a decrease in the future.

The nature of the food of the menhaden has been closely investigated. Hundreds of specimens have been dissected, and every stomach examined by me was found full of dark, greenish, or brownish mud or silt, such as occur near the mouths of rivers and on the bottoms of still bays and estuaries. When this mud is allowed to stand for a time in clear water, this becomes slightly tinged with green, indicating the presence of chlorophyl, perhaps derived from the algæ, so common on muddy bottoms. In addition to particles of fine mud the microscope reveals a few common forms of diatoms.

There are no teeth in the mouth of menhaden, their place being supplied by about 1,500 thread-like bristles, from one-third to three-quarters of an inch long, which are attached to the gill arches, and may be so adjusted as to form a very effective strainer. The stomach is globular, pear-shaped, with thick muscular walls, resembling the gizzard of a fowl, while the length of the coiled intestine is five or six times that of the body of the fish. The plain inference from these facts, taken in connection with what is known of the habits of the menhaden, seems to be that their food consists in large part of the sediment which gathers upon the bottom of still protected bays, and contains much organic matter, and also upon the vegetation that grows in such localities. Perhaps, too, when swimming at the surface with expanded jaws, they are able to gather nutritious food which floats on the water.

Their rapid increase in size and fatness, which commences as soon as they approach our shores, indicates that they find an abundant supply of some kind of food. The oil manufacturers report that in the spring a barrel of fish often yields less than three quarts of oil, while late in the fall it is not uncommon to obtain five or six gallons.

There is still some mystery about their breeding habits; thousands of specimens have been dissected since 1871 without the discovery of mature ova. In early summer the genitalia are quite undeveloped, but as the season advances they slowly increase in size and vascularity. Among the October fish a few ovaries were noticed in which the eggs could be seen with the naked eye A school of large fish, driven ashore in November in Delaware Bay by the bluefish, contained spawn nearly ripe, and others taken at Christmas-time in Provincetown harbor, evidently stragglers accidentally delayed, contained eggs quite mature. Young menhaden, from one to three inches in length and upward, are common in summer south of New York, and those of 5 to 8 inches in late summer and autumn in the southern part of New England. These are in schools, and make their appearance suddenly from the open ocean like the adult fish. Menhaden have never been observed spawning on the southern coast, and the egg-bearing individuals when observed are always heading out to sea. These considerations appear to warrant the theory that their breeding grounds are on the offshore shoals which skirt the coast from Georges Banks to the Florida Keys.

Several writers have lately expatiated on the vast fecundity of the menhaden. This has not yet been demonstrated. In a pair of immature ovaries I made out, by estimate, 10,000 eggs. A large mature fish would have many more, but in all probability the number never exceeds 40,000.

Among the enemies of the menhaden may be counted every predaceous animal which swims in the same waters. Whales and dolphins follow the schools and consume them by the hogshead, sharks of all kinds prey upon them largely; one hundred have been taken from the stomach of one shark: all the large carnivorous fishes feed upon them. The tunny is the most destructive. "I have often," writes a gentleman in Maine, "watched their antics from the masthead of my vessel, rushing and thrashing like demons among a school of fish; darting with almost lightning-swiftness, scattering them in every direction, and throwing hundreds of them in the air with their tails." The pollock, the whiting, the striped bass, the cod, the squeteague, and the garfish are savage foes. The swordfish and the bayonet-fish destroy many, rushing through the schools and striking right and left with their powerful swords. The bluefish and bonito are, however, the most destructive enemies, not even excepting man; these corsairs of the sea, not content with what they eat, which is of itself an enormous quantity, rush ravenously through the closely crowded schools, cutting and tearing the living fish as they go, and leaving in their wake the mangled fragments. Traces of the carnage remain for weeks in the great "slicks" of oil so commonly seen on smooth water in summer. Professor Baird, in his well-known and often quoted estimates of food annually consumed by the bluefish, states that probably ten thousand millions of fish, or twenty-five millions of pounds daily, or twelve hundred million millions of fish, and three hundred thousands of millions of pounds, are much below the real figures. This estimate is for the period of four months in the middle of the summer and fall, and for the coast of New England only.

Such estimates are professedly only approximations, but are legitimate in their way, since they enable us to appreciate more clearly the luxuriance of marine life. Applying similar methods of calculation to the menhaden, I estimate the total number destroyed annually on our coast by predaceous animals at a million million of millions; in comparison with which the quantities destroyed by man yearly sink into insignificance.

It is not hard to surmise the menhaden's place in nature; swarming our waters in countless myriads, swimming in closely packed, unwieldy masses, helpless as flocks of sheep, near to the surface and at the mercy of every enemy, destitute of means of defense and offense, their mission is unmistakably to be eaten. In the economy of nature certain orders of terrestrial animals, feeding entirely upon vegetable substances, seem intended for one purpose, to elaborate simple materials into the nitrogenous tissues necessary for the food of other animals which are wholly or in part carnivorous in their diet, so the menhaden, feeding upon otherwise unutilized organic matter, is pre-eminently a meat producing agent.

Man takes from the water every year eight or nine hundred millions of these fish, weighing from two hundred to three hundred thousand tons, but his indebtedness does not end here; when he brings upon his table bluefish, bonitoes, weakfish, swordfish, or bass, he has before him usually menhaden flesh in another form.

The commercial importance of the menhaden has but lately come into appreciation. Twenty-five years ago, and before, it was thought to be of very small value. A few millions were taken every year in Massachusetts Bay, Long Island Sound, and the inlets of New Jersey. A small portion of these were used for bait; a few barrels occasionally salted in Massachusetts to be exported into the West Indies. Large quantities were plowed into the soil of the farms along the shores, stimulating the crops for a time, but in the end filling the soil with oil, parching it, and making it unfit for tillage.

Since that time manifold uses have been found. As a bait fish this excels all others. For many years much the greater share of our mackerel was caught by its aid, while the cod and halibut fleet use it rather than any other fish when it can be procured. The total consumption of menhaden for bait in 1877 did not fall below 80,000 barrels, or 26,000,000 of fish, valued at $500,000. Ten years before, when the entire mackerel fleet was fishing with hooks, the consumption was much greater. The Dominion mackerel fleet buy menhaden bait in quantity, and its value has been thought an important element in framing treaties between our government and that of Great Britain.

As a food resource it is found to have great possibilities. Many hundreds of barrels are sold in the West Indies, while thousands of barrels are salted down for domestic use by families living near the shore. In many sections they are sold fresh in the market. Within six years there has sprung up an important industry, which consists in packing these fish in oil, after the manner of sardines, for home and foreign consumption. In 1874 the production of canned fish did not fall below 500,000 boxes. The discovery made by Mr. S. L. Goodale, that from these fish may be extracted, for the cost of carefully boiling them, a substance possessing all the properties of Liebig's "extract of beef," opens up a vast field for future development. As a food for the domestic animals, in the form of "fish meal," there seems also to be a broad opening.

As a source of oil, the menhaden is of more importance than any other marine animal. Its annual yield usually exceeds that of the whale (from the American fisheries) by about 200,000 gallons, and in 1874 did not fall far short of the aggregate of all the whale, seal, and cod oil made in America. In 1878 the menhaden oil and guano industry employed capital to the amount of $2,350,000, 3,337 men, 64 steamers, 279 sailing vessels, and consumed 777,000,000 of fish; there were 56 factories, which produced 1,392,644 gallons of oil, valued at $450,000, and 55,154 tons of crude guano, valued at $600,000. This was a poor year. In 1874 the number of gallons produced was 3,373,000; in 1875, 2,681,000; in 1876, 2,992,000; in 1877, 2,427,000. In 1878 the total value of manufactured products was $1,050,000; in 1874, this was $1,809,000; in 1875, $1,582,000; in 1876, $1,671,000; in 1877, $1,608,000. It should be stated that in these reports only four-fifths of the whole number of factories are included.

The refuse of the oil factories supplies a material of much value for manures. As a base for nitrogen it enters largely into the composition of most of the manufactured fertilizers. The amount of nitrogen derived from this source, in 1875, was estimated to be equivalent to that contained in 60,000,000 pounds of Peruvian guano, the gold value of which would not have been far from $1,920,000. The yield of the menhaden fishery in pounds is probably triple that of any other carried on by the fishermen of the United States. In the value of its products it is surpassed only by three: the cod fishery, which, in 1876, was estimated to be worth $4,826,000; the whale fishery, $2,850,000; and the mackerel fishery, $2,275,000; the value of the menhaden fishery for this year being $1,658,000.

In estimating the importance of the menhaden to the United States, it should be borne in mind that its absence from our waters would probably reduce all our other sea fisheries to at least one-fourth their present extent.

OBJECTS OF SEX AND OF ODOR IN FLOWERS.

A Paper read before the American Association, Saratoga, N. Y., August, 1879, by THOMAS MEEHAN, Professor of Botany to the Pennsylvania State Board of Agriculture, Germantown, Penn.

STUDENTS of nature, who have thoughtfully observed, must have noted at least two great objects in the creation of sex. The first, and leading one, is evidently to insure variation; the second, to aid and assist reproduction. But our text-books say little of the first, while every behavior of flowers is regarded as relating to the last, and hence we have so much said and written on the advantages of cross-fertilization, as if reproduction were the sole end and aim of sex.

That reproduction is not the sole end of sex is apparent from the fact that reproduction by cell division is more common in vegetation than reproduction by seed. Bulbs, tubers, rhizomes, and other subterranean structures, with bulblets, runners, and other arrangements above ground, are familiar examples. Many plants with colored corollas, seed so seldom, that never may fairly characterize them. Of these I might name *Ranunculus ficaria*, *Lilium tigrinum*, the horse radish, *Cochlearia armoracia*, etc.; and again, are those which depend on insect or similar agency for pollenization, and though apparently, as a result, bearing seed abundantly, yet rarely producing plants in nature from these seeds. Of these last I need only refer to yucca and orchideæ as the best known of the class dependent on insect fertilization. The terrestrial orchideæ of the United States mostly fruit in great abundance, and there are many thousands of seeds in each capsule, yet my researches have never been rewarded by one plant that I could believe to be a seedling; while in nearly all the cases the relation by offsets from a parent plant was plain. On the other hand, orchid locations are declining, and yucca confines its species to comparatively limited locations, apparently raising a crop of seeds more for the sake of feeding the larvæ of the yucca moth than as an aid in plant distribution. So far as reproduction is concerned, it will not be denied that millions on millions of seeds are created in vain, that thousands of millions of flowers bloom uselessly, and that volumes of odor and tons and tons of pollen are given to the winds and to the insects, without any possible benefit to the individual, which could be made to increase without any of these productions, which are of no conceivable benefit to the race, except as might arise from some imaginary good from cross-fertilization. We see from these simple considerations that sex can have but a very remote relation to the good of the individual or the race; and we may reasonably look about for some more important service which sex is to render.

We find this in variety. This is essential to our present conditions of existence. Imagine the higher order of animals increasing by division! Each would be exactly like its parent. Mr. Smith could not tell himself from Mr. Brown. But the union of two distinct individuals, and each individual with varying powers of transmitting identity, leads to infinite variety, by which each can clearly distinguish that which is his from what is his neighbor's. Variety is a greater necessity to sentient than to inanimate beings, hence we see that propagation through sex is imperative among them. But it can, in this respect, make no difference to a plant. It is of no consequence to one blade of grass that another blade should be just like it. But it is of great consequence to the animal life that is to feed on them. Each class is made to prefer some kinds of fruit and vegetables, which must have distinct characters in order to be easily recognized, and hence we have at once a good reason for form, color, fragrance, and the infinite variety these productions give rise to. If this view be correct, and I cannot conceive that it can be controverted, it puts a new view on modern teleology. In all the discussions on the various arrangements of plants and animals, we hear only of what good is to result to the individual or to the race. This is the essential character of the doctrines of natural selection. But on the principle which I have sketched out—the principle of variation—we see plants and animals not working merely for their own good, although that is incidentally involved, but for the good of generations yet unborn, and in which they can have no interest. Indeed, following the inexorable law of variation, plants may be said to be laboring to make themselves distinct from each other, so that the various animals may be better able to recognize and consume them. They must necessarily be under the control and direction of an outside power, which clearly foresees that there will be mouths, and judgment required to select the food which is to go into them; all of which would be useless, unless plants were forced into a variety, which is thus to enable them to be the more easily sacrificed when the proper time arrives. Of course, the selfish views embodied in the doctrines of modern teleology must be incidentally true. No individual would work, unless it supposed it was working for its own good. Pleasure must be a condition of existence. This also must be a universal law, and "natural selection" so far to be conceded. But this law must, of necessity, be limited. It is not for the good of a plant that it should be eaten by an animal; but it is perfectly consistent with the law of universal good that it should have just enough of thorns, or bitterness, or some other measures of defense to keep the race from being utterly annihilated.

Finally, may we not conclude that variation, and not reproduction, is the one great law to which we are primarily to refer all natural phenomena; that reproduction occupies only a place subservient to this law; and, if so, may we not proceed to review the theories which have been established under a mistaken idea of the order of things? I propose to examine, but I shall confine myself here to only one subject, indeed but to a part of that subject, namely, the relation which odor in flowers bears to the theory of cross-fertilization.

Mr. Charles Darwin, in "Cross and Self Fertilization," chap. x., p. 381, says: "We certainly owe the beauty and odor of our flowers, and the storage of a large supply of honey in them, to the existence of insects;" and Professor Asa Gray, in his recently issued "Structural Botany," p. 217, follows by observing: "Anemophilous flowers are mostly destitute of odor, and not nectariferous;" and further, p. 218: "Nor do we know that fragrance or other scent, or that nectar subserves any uses to the flowers than that of alluring insects." You see that the idea uppermost in the minds of these authors is that some direct good to the plant must be inferred from its peculiar form, color, fragrance, or secretions, and the absolute necessity of mere variation is wholly ignored. But we have color and odor even in minerals. We do not look to any special benefit to them from these possessions, but we can understand why they should possess them under the universal law of variety. Besides, odors and sweet secretions are not confined to flowers, but pervade all parts of the plant alike. The leading veins of catalpa, as recently shown by Mr. John A. Ryder, of the Philadelphia Academy of Natural Sciences, are furnished with glands which secrete nectar and furnish food for innumerable ants. We may agree with Dr. Gray that this nectar is for the purpose of alluring insects; but where does the good to the plant come in? Odor and color abound in great variety among toadstools, lichens, and seaweeds. Have these been developed to make them attractive to insects for any purposes that we can conceive of in connection with individual good? They have separate sexes, they have color, and they have odor, and they cross-fertilize; but cross-fertilization is not effected by any insect agency. If, as Mr. Darwin says, we should not have had beautiful or odoriferous flowers had insects not existed, how did these lower orders of plants come by color? We cannot understand it on any theory of natural selection, but we can understand it on the basis of the necessity for a universal variety in all things. Again, bright color is not confined to flowers. In tropical countries colored leaves abound, and of these the Begonias, Crotons, and Dracænas of our greenhouses afford familiar examples; and, strangely enough, most of these colored-leaved plants belong to classes which are supposed to be anemophilous, or fertilized by the wind, and can there-

fore have no object in making themselves attractive to insects.

But perhaps the most remarkable fact of all is, that the statement of Dr. Gray, that anemophilous plants have flowers mostly destitute of odor, is probably incorrect. Certainly there is odor in a large number of anemophilous plants. In monœcious and diœcious plants color or fragrance is usually present in the male flowers. And often both are there, but wanting in the female, unless in flowers with a conspicuous corolla, such as in cucurbitaceous plants. In these cases the degree of fragrance is equal. But odor to a greater or less degree exists in the willows, poplars, maples, rhus, spinach, Indian corn, palms, sweet chestnut, and others; but always in the male, and never in the female flowers. Instead of anemophilous flowers being mostly destitute of odor, I have not been able this year to find any male flowers of this class that have not odor, with the single exception of the common field sorrel, *Rumex acetosella*. The sweet chestnut, *Castanea Americana*, is indeed remarkable for the prodigious amount of odor and other material which, under prevailing notions of individual good, must be regarded as absolute waste, but which comes to be looked on as the height of wisdom under the laws involved in variation. As the branch grows the axillary buds, which in many plants remain dormant till spring, and then, perhaps, make a new branch, push at once and make a spike of male flowers. A bunch of these will fill a room with fragrance. There are about fifty clusters of these flowers in a spike, five flowers in a cluster, five spikes to one branch, and hence twenty-five hundred male flowers; and these all fall before the female flower with its attendant male spike is formed, and which appear at the termination of the growth instead of at the axils. There is no conceivable use for this immense crop of precocious male flowers with its attendant fragrance under any law of reproduction; but if we take into consideration the immense number of minute creatures on the earth, in the atmosphere, in water, everywhere, and the evident design of nature that they should be fed, we may understand, under the laws of variation, how even a chestnut may be made to scatter this food in profusion through the atmosphere, even though not the slightest benefit to itself or to its race should follow the act. Even the views of Professor Huxley, that the coal measures of England are the product of pollen which fell during 30,000 years in the carboniferous era, are explainable under the operation of this law of variation for the purpose of ultimate universal good, but under no theory of individual benefit from natural selection that I can see.

In pursuing our studies of the odors of flowers, we shall find many difficulties in believing that they were developed for the chief reason of attracting insects for the purpose of cross-fertilization. Not the least of these difficulties is the fact of many genera of showy colored flowers existing, which may have one or two species highly odoriferous and the rest destitute of scent. The violets of Europe are of this class. *Viola odorata* is very sweet; the pansy less so; the rest are comparatively scentless. American violets show the same characteristics. I am familiar with many species, but I only know of *Viola primulæfolia* and *Viola blanda*—two nearly allied species—that would be called sweet. Has fragrance given these sweet species any advantage in the struggle for life? If so it is, at least, not apparent. On the other hand, observation will show that the scentless flowers of these genera are just as freely visited as those which have odor. Of the many species of *Reseda* I only know of one that is fragrant, the common mignonnette. In my garden, *Reseda undata*, wholly scentless, is as freely visited by bees as its sweet sister species. Again, it is a fact that among the sweet mignonnettes some are less fertile than others, and that the least productive have the most odor. Another remarkable case in which color and fragrance are in inverse proportion to productiveness is afforded by the genus *Rubus*—the blackberry and raspberry class. *Rubus odoratus* is beautiful and fragrant. How rarely it fruits is notorious. *Rubus cuneatus* is not high colored, but it is fragrant. Not half the flowers usually produce anything, and many of those which do give but a very few carpels. *Rubus Canadensis* has very showy white flowers, but no odor, and its "berries" are always more or less defective. *Rubus villosus* is less attractive than the last, and is more perfectly productive. But the most fertile of all the species is *Rubus occidentalis*. I do not know that I ever saw a flower that did not make a perfect fruit, and yet it has no odor, scarcely any petals, and these of such a green shade of white as to be actually inconspicuous. On the ground of variety, in which fragrance is to play its part, and which must of necessity permeate all things, we can understand its uses; but we are lost when we attempt to explain such facts as these by any hypothesis that has for its foundation mere individual good.

May we not, then, logically say that sex in nature is not primarily for reproduction, but to insure variation; that questions which properly come under this law of variation have but a remote relationship to questions of natural selection, but are referable to some external power governing universal good, with which the individual governed has nothing to do, and which as often tend to the destruction of individuals or races as to their preservation?

ON THE PHENOMENA OF HEATING METAL IN VACUO BY MEANS OF AN ELECTRIC CURRENT.

By THOS. A. EDISON.

Read before the American Association, Saratoga Springs, 1879.

IN the course of my experiments on electric lighting, I have developed some striking phenomena arising from the heating of metals by flames and by the electric current, especially wires of platinum, and platinum alloyed with iridium. These experiments are still in progress.

The first fact observed was that platinum lost weight when heated in a flame of hydrogen, that the metal colored the flame green, and that these two results continued until the whole of the platinum in contact with the flame had disappeared.

A platinum wire four thousandths of an inch in diameter, and weighing 306 milligrammes, was bunched together and suspended in a hydrogen flame. It lost weight at the rate of a fraction less than one milligramme per hour as long as it was suspended in the flame.

When a platinum wire is stretched between two clamping posts and arranged to pass through a hydrogen flame, it is colored a light green, but when the temperature of the wire is raised above that of the flame, by passing a current through it, the flame is colored a deep green.

To ascertain the diminution in the weight of platinum wire, when heated by the electric current, I placed between two clamping posts a wire five thousandths of an inch in diameter and weighing 266 milligrammes. This wire, after it was brought to incandescence for twenty minutes by the current, lost one milligramme. The same wire was then raised to incandescence for twenty minutes more, and it gave a loss of 3 milligrammes; afterwards it was kept incandescent for one hour and ten minutes, at which time it weighed 258 milligrammes—a total loss of 8 milligrammes. Another wire weighing 343 milligrammes, was kept moderately incandescent for nine consecutive hours, after which it weighed 301 milligrammes, showing a total loss of 42 milligrammes.

A platinum wire 1-20-thousandth of an inch in diameter was wound in the form of a spiral one-eighth of an inch in diameter, and ½ an inch in length; the two ends of the spiral were secured to clamping posts, and the whole apparatus was covered with a glass shade 2½ inches in diameter and 3 inches high. Upon bringing the spiral to incandescence for twenty minutes, that part of the globe in line with the sides of the spiral became slightly darkened; in five hours the deposit became so thick that the incandescent spiral could not be seen through the deposit. This film, which was most perfect, consisted of platinum, and I have no doubt but large plates of glass might be coated economically by placing them on each side of a large sheet of platinum kept incandescent by the electric current.

This loss in weight, together with the deposit upon the glass, presented a very serious obstacle to the use of metallic wires for giving light by incandescence, but this was easily surmounted after the cause was ascertained.

I coated the wire forming the spiral with the oxide of magnesium, by dusting upon it finely powdered acetate of magnesium while incandescent. The salt was decomposed by the heat, and there remained a strongly adherent coating of the oxide. This spiral so coated was covered with a glass shade and brought to incandescence for several minutes, but instead of a deposit of platinum upon the glass, there was a deposit of the oxide of magnesia. From this and other experiments I became convinced that this effect was due to the washing action of the air upon the spiral; that the loss of weight and the coloration of the hydrogen flame were also due to the wearing away of the surface of the platina, by the attrition produced by the impact of the stream of gases upon the highly incandescent surface, and not to volatilization, as commonly understood; and I venture to say, although I have not tried the experiment, that metallic sodium cannot be volatilized in high vacua by the heat derived from incandescent platinum; any effect that may be produced will be due to the washing action of the residual air. After the experiment last described, I placed a spiral of platinum in the receiver of a common air pump, and arranged it in such a manner that the current could pass through it while the receiver was exhausted. At a pressure of two millimeters, the spiral was kept at incandescence for two hours before the deposit was sufficient to become visible. In another experiment, at a higher exhaustion, it required five hours before a deposit became visible.

In a sealed glass bulb exhausted by a Sprengel pump to a point where a ¼ of an inch spark from an induction coil would not pass between points one millimeter apart, was placed a spiral, the connecting wires passing through the glass. This spiral has been kept at the most dazzling incandescence for hours without the slightest deposit becoming visible.

I will now describe other and far more important phenomena observed in my experiments.

If a short length of platinum wire, one thousandth of an inch in diameter, be held on the flame of a Bunsen burner, at some part it will fuse, and a piece of the wire will be bent at an angle by the action of the globule of melted platinum; in some cases there are several globules formed simultaneously and the wire assumes a zigzag shape.

With a wire four thousandths of an inch in diameter this effect does not take place, as the temperature cannot be raised to equal that of the smaller wire, owing to the increased radiating surface and mass. After heating, if the wire be examined under a microscope, that part of the surface which has been incandescent will be found covered with innumerable cracks. If the wire be placed between clamping posts, and heated to incandescence for twenty minutes, by the passage of an electric current, the cracks will be so enlarged as to be seen with the naked eye; the wire under the microscope presents a shrunken appearance, and is full of deep cracks. If the current is continued for several hours, these effects will so increase that the wire will fall to pieces.

This disintegration has been noticed in platina long subjected to the action of a flame by Prof. John W. Draper. The failure of the process of lighting, invented by the French chemist, Tessié-du-Motay, who raised sheets of platinum to incandescence by introducing it into a hydrogen flame, was due to the rapid disintegration of the metal.

I have ascertained the cause of the phenomenon, and have succeeded in eliminating that which produces it, and in doing so have produced a metal in a state hitherto unknown and which is absolutely stable at a temperature where nearly all substances melt or are consumed—a metal which, although originally soft and pliable, becomes as homogeneous as glass and as rigid as steel. When wound in the form of a spiral is as springy and elastic when at the most dazzling incandescence as when cold, and which cannot be annealed by any process now commonly known.

The cause of this shrinking and cracking of the wire is due entirely to the expansion of the air in the mechanical and physical pores of the platinum and the contraction upon the escape of the air.

Platinum as sold in commerce may be compared to sandstone in which the whole is made up of a great number of particles with many air spaces. The sandstone upon melting becomes homogeneous and no air spaces exist. With platinum or any metal, the air spaces may be eliminated and the metal made homogeneous by a very simple process; this process I will now describe.

I had made a large number of platinum spirals, all of the same size and from the same quality of wire. Each spiral presented to the air a radiating surface of 3-16ths of an inch. Five of these were brought by the electric current up to the melting point, the light was measured by a photometer, and the average light was equal to four hundred standard candles for each spiral just at the melting point.

One of the same kind of spirals was placed in the receiver of an air pump, and the air exhausted to two millimeters. A weak current was then passed through the wire to slightly warm it for the purpose of assisting the passage of the air from the pores of the metal into the vacuum. The temperature of the wire was gradually augmented at intervals of ten minutes, until it became red. The object of slowly increasing the temperature was to allow the air to pass out gradually and not explosively. Afterwards the current increased at intervals of fifteen minutes. Before each increase in the current, the wire was allowed to cool, and the contraction and expansion at these high temperatures caused the wire to weld together at the points previously containing air. In one hour and forty minutes this spiral had reached such a temperature without melting that it was giving a light of twenty-five standard candles, whereas it would undoubtedly have melted before it gave a light of five candles, had it not been put through the above process. Several more spirals were afterwards tried with the same result. One spiral which had been brought to these high temperatures more slowly gave a light equal to thirty standard candles. In the open air this spiral gave nearly the same light, although it required more current to keep it at the same temperature.

Upon examination of these spirals which had passed through the vacuum process by the aid of a microscope, no cracks were visible; the wire had become as white as silver, and had a polish which could not be given it by any other means; the wire had a less diameter than before treatment, and it was exceedingly difficult to melt in the oxyhydrogen flame as compared with untreated platinum; that it was as hard as the steel wire used in pianos, and that it could not be annealed at any temperature.

My experiments with many metals treated by this process has proved to my satisfaction, and I have no hesitation in stating, that what is known as annealing of metals to make soft and pliable, is nothing more than the cracking of the metal—in every case where a hard-drawn wire had been annealed, a powerful microscope revealed myriads of cracks in the metal.

Since the experiments of which I have just spoken, I have, by the aid of Sprengel mercury pumps, produced higher exhaustions, and by consuming five hours in excluding the air from the wire, and intermitting the current a great number of times, succeeded in obtaining a light of eight standard candles from a spiral of wire with a total radiating surface of 1-32 of an inch, or a surface about equal to a grain of buckwheat. With spirals of this small size which have not passed through the process, the average amount of light given out before melting is less than one standard candle. Thus I am enabled by the increased capacity of platinum to withstand high temperature to employ small radiating surfaces, and thus reduce the energy required per candle light. I can now obtain eight separate jets, each giving out an absolutely steady light, and equal to sixteen standard candles, or a total of 128 candles, by the expenditure of 30,000 foot-pounds of energy, or less than one horse-power.

As a matter of curiosity, I have made spirals of other metals, and excluded the air from them in the manner stated. Common iron wire may be made to give a light greater than platinum not treated; the iron becomes as hard as steel, and just as elastic. Nickel is far more refractory than iron. Steel wire used in pianos becomes decarbonized, but remains hard and assumes the color of silver. Aluminum melts only at a white heat.

In conclusion, it may be interesting to state that the melting point of many oxides is dependent upon the manner of applying the heat; for instance, pure oxide of zirconium dose not fuse in the flame of the oxyhydrogen blow-pipe, while it melts like wax and conducts electricity when on an incandescent platinum spiral, which is at a far lower temperature. On the other hand, oxide of aluminum easily melts in the oxyhydrogen flame, while it only vitrifies on the platinum spiral.

HOMOLOGIES IN THE LAURACEÆ.

Paper read before the American Association, Saratoga Springs, 1879, by LESTER F. WARD.

THE flowers of the *Lauraceæ*, although inconspicuous and unattractive, are interesting upon a close study. The interest chiefly arises from the presence within them of numerous so-called glands or functionless organs. In some cases the shape of these glands so strikingly suggests the anther that we can scarcely hesitate to pronounce them rudimentary stamens. In other cases they consist of mere filaments or staminodia, which, as we shall see, may be abortive petals. In the male flower of *Sassafras officinale*, which is always diœcious, there are six glands, all with anther-shaped heads on short filaments, arranged in pairs in a single whorl or row, each pair being joined at their base by a thin connective, out of the summit of which between the glands arises one of the three inner stamens, thus:

The floral envelope, which is considered monochlamydous, consists of a six-lobed calyx with a very short tube, the divisions of the limb being so arranged that there are three outer and three inner, alternately imbricating over each other, thus:

There are nine true stamens, which are arranged in three rows of three each, and inserted on the tube below the lobes, the outermost row opposite the outer lobes, the middle row opposite the inner lobes, and the inner row again opposite the outer lobes, but on the connective between the pairs of glands as above shown. The pistil is wholly wanting.

In the female flowers of sassafras there are also six glands, which have more the appearance of stamens than those of the male flowers. They are situated opposite the sepals in a single row. These have been denominated rudimentary stamens by high authority, and there can be no doubt that they are such, although the anther-like heads now consist of homogeneous tissue without cells. True stamens in the female flowers are wholly absent.

The male flowers of *Lindera benzoin*, a closely allied genus, are similar to those of sassafras in all respects except that they possess a rudimentary ovary. The female flowers of lindera, however, differ from the female flowers of sassafras in an important particular. Prof. Gray describes them as possessing "fifteen to eighteen rudiments of stamens in two forms." The normal number, however, is just fifteen, a greater number being sometimes found in the same way

that we sometimes find eight instead of six calyx-lobes. As regards the "two forms," nine of the fifteen are without glandular heads, and these are arranged in precisely the same manner as are the true stamens of the male flowers both of sassafras and of lindera. The remaining six have the anther-like heads, and are arranged in pairs like those of the male flowers.

Now, it is not probably doubted by any one that at one time the flowers of these two genera were perfect, *i.e.*, hermaphrodite, and this sufficiently accounts for the obsolete stamens of the female flowers of sassafras, but it does not explain why there are only six of these when the male flower has nine perfect stamens. Neither does it supply a reason for the six very similar glands of the male flower in addition to the true stamens. Still less does it account for the nine glandless and six gland-bearing rudiments in the female flowers of lindera.

In view of all the facts I, therefore, conclude:

1. That the six glands in the staminate flowers of both genera are the true homologues of the six rudimentary stamens in the fertile flowers of sassafras, and of the six gland-bearing rudiments of stamens in the fertile flowers of lindera.

2. That the nine polleniferous stamens of the male flowers of both genera are transformed petals of a former stage, in which the flowers were dichlamydous.

3. That the nine glandless filaments in the female flowers of lindera are the true homologues of the present stamens of the male flowers, which may or may not have once possessed anthers also.

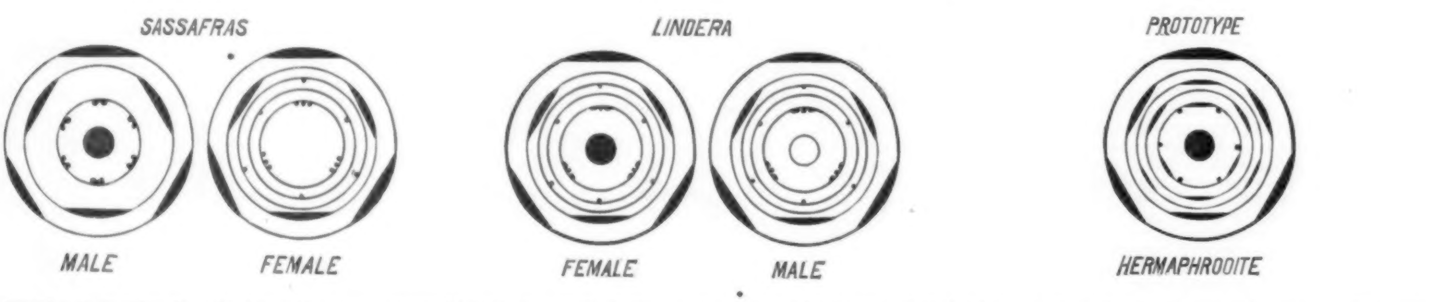
4. That at the time when the present stamens were petals the present gland-bearing rudiments were the true stamens.

The flowers of the *Lauraceæ* would therefore seem to have passed through a two-fold transmutation, having their petals first transformed into stamens, at least in one (the male) form, and then gradually losing their original stamens by their conversion into rudimentary glands. These not being injurious, but rather affording some protection, have not disappeared altogether, but remain very conspicuous objects. It is still a question whether to regard the nine glandless filaments in the female flowers of lindera as abortive stamens or as the remains of the original petals; in other words, whether the petals in the female flowers were ever really transformed into stamens, or whether they simply disappeared in sassafras and are now disappearing in lindera. I am disposed to adopt the latter view.

The peculiar connective joining the rudimentary stamens with the inner row of true stamens and of rudimentary petals in the female flowers of lindera may be explained by supposing that the original six stamens were situated one on each side of the three inner petals and inserted with them on the tube of the calyx.

The two genera which I have described, therefore, and perhaps all the plants of the laurel family, must be supposed to have descended from a common progenitor in the remote past, which differed in some important respects from any plants now existing. This primordial ancestor of the Lauraceæ possessed dichlamydous and eleutheropetalous, hermaphrodite flowers, with a six parted calyx, the divisions arranged as now, nine narrow petals inserted in three rows on the calyx tube, the outer and inner rows opposite the outer lobes, and the intermediate row opposite the inner lobes; and six stamens inserted at the base of the inner petals, one on each side.

The following diagrams, representing cross sections of each of the forms now found, and also of the original from which all are supposed to have been derived, will serve to illustrate the progress of the changes which have taken place. The inner circle represents the gynæcium, which is not drawn when wanting, and not filled out when abortive. The next three whorls embrace the present andrœcium, as also the primordial petals. In the female of sassafras only one of these (the innermost) occurs representing the primordial andrœcium, now the anther-like glands. The two outermost whorls denote the lobes of the calyx. In the ancestral form the nine petals are represented as somewhat ligulate, a stamen on each side of each of the three inner ones.



At first view it may be thought that this is an extreme and complicated metamorphosis for a flower to undergo, but it must be remembered that a vast period of time has been required to effect it. We know as well as we know anything about plants, that the flowers of many of them are actually in a transition state. The number of species that any botanist can call to his mind in which the sexes are only partially separated and apparently in a vacillating condition between hermaphrodism and diclinism is very great. They occur in families the most widely separated botanically, and illustrate all shades of the process. Even lindera, which we have been studying, differs from its close relative, sassafras, in this respect. The rudimentary pistil in the male, and the rudimentary petals (or stamens) in the female flowers, both of which have long since disappeared from sassafras, plainly show that the latter is, so to speak, a much older genus, and commenced its career of transmutation ages further back in geologic time; and we may also with some certainty predict that lindera, too, will one day lose these useless vestiges. Nor are we restricted for time in the case of the *Lauraceæ*. Vegetable palæontology teaches that this is one of the oldest families of flowering plants. Its fossil remains are found deep down in Mesolithic strata. Prof. Lesquereux detects no less than seven genera of this order in the Dakota group of our American cretaceous formation. Among these occur a number of species of sassafras, but the absence of lindera is quite significant in view of its morphology, as above pointed out, indicating a more recent origin. These plants have therefore had, at least, the entire cretaceous, tertiary, post tertiary, and modern periods in which to undergo these changes, so that we only need assume the same measure of differentiation which we are certain is now taking place, to account for any modification which their present morphology may require to have taken place in the past.

INFLUENCE OF ELECTRICITY ON COLLIDING WATER DROPS.

Lord Rayleigh communicates a paper to the Royal Society on this subject. It has been long known that electricity has great influence on fine jets of water ascending in a nearly vertical direction. In its normal state a jet resolves itself into drops, which even before passing the summit of the column, and still more after passing it, are scattered to a considerable width. When a feebly electrified body is brought into the neighborhood of the jet, it undergoes a remarkable transformation, and appears to become coherent; under more powerful electrical action the scattering becomes more marked than at first. The latter action is due to mutual repulsion of the drops; the former has been hitherto unexplained. The cohesion seems to be more apparent than real; the seat of sensitiveness is at the place of resolution into drops; each drop carries away with it an electric charge, which can be collected in an insulated receiver. He is able to show that the normal scattering is due to the rebound of the colliding drops; such collisions being inevitable in consequence of the different velocities acquired by the drops under the action of capillary force, as they break away irregularly from the continuous portion of the jet. When the resolution is regularized by the action of external vibrations, as in Savart's and Plateau's experiments, the drops must still come into contact as they reach the summit of their parabolic path. Under moderate electrical influence, instead of rebounding after collision, they coalesce, and the jet is not scattered. This behavior of the drops becomes apparent under instantaneous illumination, such as that of an induction coil, into the secondary circuit of which a Leyden jar is introduced. To obtain further evidence two similar jets were made to collide horizontally, one being in communication with the earth, the other supplied from an insulated cistern. The sensitiveness to electricity was extraordinary. A piece of rubbed gutta percha brought near the insulated bottle at once determined coalescence. It was also possible to cause the jets again to rebound from one another, and then to coalesce.

Besides statical electricity, the electro-motive force of a single Grove cell was sufficient to produce the same phenomena, one pole being connected with the water, the other to earth. Even the discharge of a condenser charged by a single Grove cell answered the purpose. The writer indicates in conclusion the probable application to meteorology of the facts mentioned. The formation of rain must obviously depend materially on the consequences of encounters between cloud-particles. If the contacts result in coalescence the drops must rapidly increase in size and be precipitated as rain. We may thus anticipate an explanation of the remarkable but hitherto mysterious connection between rain and electrical manifestations.

ROTATION OF THE PLANE OF POLARIZATION BY ELECTRO-MAGNETISM IN A VAPOR.

MM. Kundt and Rontgen have communicated their results on this subject to the Munich Academy. They have proved the fact of such rotation, which Faraday failed in demonstrating, at least in the vapor of sulphide of carbon. This substance was chosen for experiment because it shows strong electro-magnetic rotation in the liquid state, and because its vapor has great tension, even at moderate temperatures. An iron tube, closed at the ends by thick glass plates, was inclosed in an outer tin tube through which steam at 100° Cent. could be passed. The outer tube was surrounded by six large coils of wire, containing 400 turns of thick wire each, through which a current from 64 large Bunsen elements was passed. Sulphide of carbon vapor was heated as above described, and at the temperature of boiling water the vapor became transparent. A beam of polarized light was passed through the tube, and analyzed by Nicol's prism. On the current being sent, a distinct brightening of the field was observed, which increased, if, after rotating the prism afresh to darkness, the current was reversed. The rotation was in the direction of the positive current through the coils, and amounted to about half a degree. Sulphuric ether similarly treated gave no result. The authors of the paper are making apparatus by means of which to examine permanent gases and unsaturated vapors, with the special view of ascertaining whether oxygen rotates the plane of polarization in the same direction as other gases. Monsieur E. Bichat contributes a memoir to the *Comptes Rendus* of the Academy, in which he states that he has been following out a similar series of experiments since July, 1878, which give very different results from those noted above. He points out that the iron tube used by the German physicists very materially injures the completeness of the action. It is in reality a hollow electro-magnet; whereas his tube being of brass allows the whole force of the current to expend itself on the contained vapor. In illustration of the difference, he passes a tube containing liquid bisulphide of carbon through the two poles of an electro-magnet, obtaining a rotation of 10° 30′; but on using a single hollow core with the same current, no appreciable rotation is obtained. If the iron tube be thin, the action is not entirely annulled, but is considerably diminished—in the instance he gives from 5° to 1°. With his apparatus he has followed from zero to boiling point the rotation of the same current in sulphide of carbon and bichloride of tin. The molecular rotatory power is maintained as long as the boiling point is not approached. At that moment there is a diminution much more rapid than could have been foreseen from calculations based on the ratios of density.

JAMIN'S ELECTRIC LAMP.

The annexed engraving is a representation of Jamin's electric lamp. It consists of two nearly parallel carbon rods, surrounded by an elliptical coil of wire, through which the current passes. It is this arrangement of the conductor in the shape of a coil around the rods that forms the novel part of the invention, and gives rise to the phenomena witnessed. The coil, being in the same vertical plane as the carbon rods, is arranged so that the current through it is in the same direction as that which flows through the latter, producing the arc at their extremities. In virtue of the law that currents in the same direction attract and those in opposite directions repel each other, the currents through the upper portion of the coil will attract the arc, and those through the lower portion will repel it. The lateral currents also, by reason of their tendency to deflect the arc into parallelism, aid in repelling the latter to the extremities of the carbons. So powerful is this effect of repulsion that, if the number of turns of wire in the coil be too great, the arc, if caused to pass between the lower portions of the carbon rods, will move upward with great velocity, and the light becomes extinguished, owing to the arc being too strongly attracted in the direction of the extremities of the carbons. With this apparatus the arc becomes strongly curved; and it is stated that the light evolved is very considerably augmented by its use, owing to the carbons being no longer consumed laterally, so as to shade the light. By using this apparatus, also, the lamp may be inverted, without any danger of the arc quitting the extreme ends of the carbon rods.—*Eng. Mechanic.*

AN OPTICAL ILLUSION.

Mr. R. A. Proctor writes in the *Newcastle Chronicle:*

Form a tube of card or paper some nine or ten inches long and an inch or so in diameter. Look through this at any object with either eye, holding a hand in front of the other eye close by the farther end of the tube, in such a way that the middle of the hand hides from the view of that eye the object seen through the tube with the other. Then, when both eyes are open, you seem to see the object through a round hole in your own hand; and looking scrutinizingly at this singular opening you perceive that it is lined with card or paper. I am not sure that the following explanation is much more than a restatement of the fact in a somewhat roundabout way—yet it does, perhaps, present the *rationale* of the matter as clearly as can be done in default of any satisfactory explanation of vision itself. (For, after all, it must be remembered that we can no more explain the form of sensation which we call vision than we can explain the form of attraction which we call gravity.)

We can recognize the nature of vision, and we can perceive how the law of optics follow, just as we recognize the attraction of gravity and perceive thence how the laws of planetary motion follow. But we cannot tell why the law of gravity exists, and we are equally in the dark as to the true significance of vision. We see with both eyes but one field of view. Ordinarily, this field includes, without confusion, what is seen by both eyes; there is not any confusion ordinarily even when the two pictures are perfectly distinct, as when the range of vision includes near objects and distant ones (for then, as any one will find who shuts alternately the right and left eye, the arrangement of the objects seen with either eye is entirely different, and actually different objects are visible with one and the other eye). But this power of adaptation is not unlimited, and the experiment described by Mr. Barkas, like the one I have described, taxes the power unduly. The real wonder is, *not* that the results of such experiments are so strange, but that results almost as strange are not constantly resulting from binocular vision.

The single field of view *must* include what is seen with one eye—namely, a small bright picture, limited by the dark (because shadowed) inclosure of the tubing. The field of view must also include what is clearly seen with the other eye, that is in the art gallery experiment the plank's surface, and, in the one I have described, the hand. Necessarily then the small bright image is seen on a background of plank in one case, and of hand-surface in the other, and, being obviously remote, seems to be seen through the plank, or through the hand. That the tint of plank, or hand, is entirely lost over the small field of view really belonging to the eye which looks through the tube, can be understood when we remember that, as this eye is shielded from the light, the pupil probably dilates, and apart from the inherent luster of the object seen in this way, the eye, owing to this change, sees it as a brighter object. It is indeed noteworthy that as a rule the mind, in dealing with optical impressions, refuses to combine two impressions belonging to the same part of the field of vision, even though both eyes may help to convey them. Thus, as Ruskin has pointed out, I think, if we notice the images of clouds in clear water, we do not at the same time see the stones and other objects at the bottom of the water; if we note these, we do not see the clouds. I may remark that in talking about binocular vision (though I believe I have here reasoned correctly), I am somewhat like blind Prof. Sanderson discussing the laws of optics. For the difference between the focal lengths of my two eyes is so great that I have never seen any object with both eyes at once, and probably I never shall. If I bring the two eyes to the same focal length, by using a very strong concave glass for my very short-sighted left eye, leaving the other eye unaided, I simply see all objects double, and so absurdly distinct in their duplicity that no amount of reasoning will correct the impression. It is too late for me now to try to teach these unequal eyes of mine to work together—the use of a concave glass, as above described, causing giddiness and pains in both eyes in a very short time. The complete disregard which either eye has of the work of the other is singular, and illustrates effectively some of the points noted above. When I am looking at distant objects, the left eye, though open, shows nothing (unless there happens to be a bright light in view on a dark ground, in which case a blurred image of the light is seen to the left of its position as seen distinctly with the right eye). On the other hand, when I am reading with the left eye (as I usually do, though not always) the right is perfectly at rest, though open. Once or twice, by the way, some rather amusing experiences have resulted from this remarkable difference, as when persons who have seen me reading with the page five inches from my eyes have taken it for granted I was exceedingly short-sighted, whereas with my right eye I have as good sight as most people for distant objects.

A MIRROR BAROMETER.

M. LEON TESSERENC DE BORT has ingeniously modified the common aneroid barometer by substituting for the train of clock-work terminating in a pointer a mirror mounted on a jeweled axis, which is rotated by the rise and fall of the exhausted receiver, and its indications read off by a small telescope by reflection from a graduated scale. The sensibility of the instrument is said to be much increased, and all errors due to a long train of wheelwork are eliminated.

ON THE DIFFERENT METHODS OF ARTIFICIAL ALIMENTATION.*

By THOMAS J. GALLAHER, M.D., of Pittsburg.

I PROPOSE to make a few remarks on the different methods of administering food aside from the voluntary method by the mouth. I propose also to say something of the substances to be employed in these various methods. This subject has not been treated with sufficient fullness in systematic treatises; but it has received considerable attention in medical periodical literature.

FEEDING BY INJECTIONS.

The most common method of artificial alimentation consists of injections into the rectum. Beef tea and broths of various kinds have been employed in this way from time immemorial, and instances innumerable are given where life has been sustained by these for long periods. Recent observations, however, do not fully confirm the great estimate put on these preparations as formerly employed.

It is now taught by many that the albuminous, caseous, and fatty elements of such enemas, without previous digestion, cannot be absorbed by the large intestines, and that, when these substances are introduced into the rectum unchanged, they are precipitated against the walls of the bowel, where they undergo fermentation and cause flatulency and pain. Dr. W. B. Richardson asserts that fatty substances, even after emulsification, cannot be absorbed because of the absence of lacteal vessels in the large intestine, by which alone they can be taken up.

Before finishing the subject of beef-tea injections, I will give some interesting experiments bearing on the subject of rectal digestion and absorption, from which imperfect conclusions may be drawn.

Experiments in reference to this subject were recently made in Germany by Czerny and Latchenberger. Five fistulous openings having formed in the lower part of the colon, in a gentleman who had long suffered from scrotal hernia which had inflamed and ulcerated, offered a good opportunity. The portion of the bowel experimented on extended from the lower of the fistulous openings in the groin to the sphincter muscles of the arms, a distance of twelve inches. It embraced therefore the rectum and a few inches of the sigmoid flexure of the colon. When water alone was injected, from 617 to 772 grains were absorbed in seven hours. The white of hard-boiled eggs cut into small fragments, shreds of fibrin, soluble albumen, starch, and fats did not dissolve or undergo any digestive changes.

The subjection of these substances to artificial digestion in mucus obtained from this part was attended by a similar result. Bits of the white of hard-boiled eggs inclosed in perforated capsules were introduced through the fistula into the bowel and retained there ten days without the slightest evidence of solution. It was observed, however, that all of these substances when previously subjected to artificial digestion, were absorbed.—(*American Journal of Medical Science*, April, 1874, from *Lancet*.)

ARTIFICIAL DIGESTION AND FOODS.

Dr. Williams relates a case of supposed gastric ulcer, where the weight of the patient was maintained by nutritive injections for a period of ten days. Beef tea, eggs, and brandy were the articles employed.—(*American Journal of Medical Science*, January, 1875, from *Lancet*.)

Some interesting experiments were made by Dr. Leube, of Erlangen, in 1872, chiefly upon dogs. He combined the pancreas of a pig or ox with various articles of food, and injected the mixture into the rectum of the dog. He found that digestion was performed and the nutritious elements absorbed.

The dog, by this method of feeding, retained its flesh and strength, while the evacuations were of proper consistence and smell. Applying the knowledge thus acquired to the human subject, he met with similar success by injecting into the rectum similarly prepared articles of food.

His experience enabled him to formulate the following preparation, which he preferred for anal injections: From $2\frac{1}{2}$ to 3 ounces of the pancreas of the pig or ox, carefully deprived of its fat and finely minced, were combined with from 5 to 9 ounces of beef, also finely minced and grated. These articles were well triturated together in a mortar with the addition of warm water, until the whole acquired the consistence of thick soup. The bowels having been evacuated an hour before, the whole amount was injected into the rectum at once with a syringe having a wide nozzle. The presence of this mass in the bowel, as a rule, produced no uneasiness, but would often remain from twelve to thirty-six hours without giving rise to a stool. Fat and starch were frequently added to the preparation before injecting.

Two cases, one affected with malignant disease at the upper part of the intestinal tube, and the other with gastric ulcer in which no food whatever could be retained upon the stomach, were subjected to this method of alimentation for a long time.

It proved, in his opinion, superior to all other means in maintaining the strength and flesh of the patients. The pulse became stronger and there was an improvement in the general condition and spirits of the patient.—("Half-Yearly Abstract," July, 1872, page 146.)

Recurring to beef tea as an article for rectal alimentation, the experiments of Czerny and Latchenberger show that neither fat, albumen, fibrin, starch, nor any saccharine substance which it may contain, can be digested to any extent by the rectal fluids; and that these substances as such cannot therefore gain entrance to the system by the rectum. It is true this bowel contains some follicles of Lieberkuhn and other glands by which a small amount of mucus and digestive fluid is normally secreted; but the amount is so small that its influence on digestion practically amounts to nothing. What then becomes of the beef tea or animal extracts when thrown unaltered into the bowels? The water, organic saline principles, and inorganic salts are certainly absorbed without change. These, of course, afford some nutrition to the body. But the albumen, fibrin, casein, and fat remain in the bowel for a while and then pass away. These substances must be digested artificially either outside of the body or while in the rectum by means of rennet, pepsine, pancreatine, etc., before they are in condition to enter the blood. After complete digestion the albuminoids are readily admitted to the circulation by capillary absorption, while the fatty matter can only be taken up to a very moderate extent. It seems certain that the existence of lacteals is not essential to a limited absorption of fatty emulsions.

In the preparation of this tea the meat should be cut very fine and allowed to macerate in cold or tepid water for not less than four or six hours. One pound of beef to one pint of water is the proportion generally employed. The whole may be boiled for thirty minutes or more. Some prefer using it uncooked. When sufficiently cooled, some of the digestive principles already alluded to should be added, and the whole gently agitated for an hour or so, when it is ready for use. Pepsine recently prepared from the stomach of the pig, calf, or sheep, according to the British formula, is preferable to the article sold in the shops.

As to Leube's preparation of grated raw meat, intimately combined by trituration with the pancreas of the pig or ox, I cannot speak in approving terms. I employed it on one occasion with uncertain results. Aside from the uncertain action of the pancreatic substance and juice on the albuminous ingredients of the meat, the difficulty experienced in making the preparation will always be an obstacle to its general adoption.

Defibrinated blood has been recently introduced as a valuable article for rectal alimentation. From 4 to 5 ounces are recommended to be injected at a time, to be repeated several times daily. Dr. A. H. Smith, in a report read before the New York Therapeutical Society, August 12, 1878 (*New York Medical Journal*), speaks of it in encouraging terms. Many of the elements of this injection will of course be admitted to the circulation, through the coats of the large intestine, but whether the serum and metalbumen, modifications of common albumen, can be absorbed without previous digestion, is a matter that can only be settled by further research.

Many cases come to the notice of the physician where, from stricture of the œsophagus, disease of the throat or mouth, or obstinate refusal on the part of the patient to take food, a sufficient amount of nourishment cannot be given to sustain life.

Where anal or renal disease exists, in conjunction with such cases, food cannot be introduced by the bowel. Other methods of alimentation must then be resorted to. A common expedient is to force the food into the stomach through the œsophagus tube. Dr. B. W. Richardson prefers the double to the single canula for this purpose, for by it food may be passed into the stomach by one chamber while the gastric gases are escaping by the other. In the use of the single tube these gases often interfere with the feeding.

FEEDING BY THE NOSE.

In consequence of the difficulty and sometimes danger of giving food in this way, feeding by the nose has been suggested and successfully carried into effect. For this purpose a syringe holding from 2 to 4 drachms may be employed. The patient, should he resist, must be placed in proper position, and held till the food is administered. One syringeful should then be gently injected into the nostril, then another, and another, until the amount desired be passed into the stomach. Time should be given between each syringeful for the patient to swallow and breathe.

All kinds of digestible food that can be reduced to a liquid or semi-solid state may be given in this way. In persons who offer no resistance, no difficulty whatever is experienced. The patient, however, must always lie upon his back, with head inclined backward and chin slightly elevated at every nasal feeding. Small children, even those at the breast, may easily be fed in this way.

Dr. Alexander Moxey, who was among the first, if not the very first, to call the attention of the profession (especially those engaged in the management of the insane) to the value of feeding by the nose, prefers passing the food through a funnel. After placing the patient in a proper position, he inserts the little end of a small Wedgwood-ware funnel just within the nostril, and gently pours the food into it, in small portions (two or four drachms) at a time. One pint or more may be introduced at one feeding, and the operation repeated two or three times a day, or as often as may be necessary. The mucous membrane of the nostril does not become irritated by the passing of food over it. Cold water causes more uneasiness than food. The articles should always be used while warm. For insane persons and those who are determined to commit suicide by starvation, this method is especially applicable. Many of these will take food in the natural way after having been subjected to this process a few times.

Dr. Tennant advocates the practice of feeding fever patients by the nose, and Dr. Hinchman favors nasal feeding among the obstinate insane.

This method has advantages over all others in many cases. The patient can always be easily restrained so that the nurse can operate with success, and the right kind of food can always be carried to the place where digestion and absorption are assured, with little or no risk to the patient. (*Lancet*, May, 1869, American edition.)

HYPODERMIC FEEDING.

Since the discovery of administering medicine by hypodermic injections, attempts have been made to administer food by the same process. Stricker and Aser some years ago injected peptones beneath the skin with the view of absorption and nutrition, and more recently Munzel and Perco, of Germany, succeeded in administering subcutaneously substances that had not undergone artificial digestion. Dogs were the chief animals operated on, but in one instance they injected nine grains of undigested food beneath the skin of a man. In these experiments fatty substances, such as olive oil, train oil, almond oil, etc., and subsequently milk, yolk of eggs, and dissolved sugar, were employed. The milk, eggs, and sugar were quickly absorbed, while the fatty matters disappeared more slowly. After 48 hours no trace of fat was discoverable. From 1 to 8 drachms of oil were injected at a time. (*American Journal of Medical Science*, October, 1869.)

Dr. James T. Whittaker reports a case of supposed gastric ulcer in a young man of twenty, in which nourishment was given exclusively by injections beneath the skin for a period of six days. Milk alternating with extract of beef was at first given, but in a day or two cod liver oil was substituted and used exclusively. From 2 to 3 ounces of oil were injected daily, and on one day as much as 4 ounces were given. No pain or uneasiness was occasioned by the presence of the oil, but two small abscesses arose from the milk injections. (*American Journal of Medical Science*, April, 1877.)

The most important and successful case of artificial feeding through the skin on record is reported by Dr. Kreng. The patient was an insane Hungarian, aged fifty-seven years. Having refused food, the doctor was obliged, for a period of 27 months, to support him by injections into the stomach through the usual œsophagal tube. At the end of this period a violent cough, an ejection of the food along the side of the tube, and threatening suffocation, attended every effort at giving food.

It was now determined to administer food subcutaneously. Accordingly from 15 to 20 cubic centimeters of olive oil were injected beneath the skin twice daily for a period of twenty days, no other nourishment having been given during that time. The oil certainly furnished him with nourishment, for he retained his flesh and strength. At the end of this period he ate without compulsion for some time, but on a short relapse occurring the injections were resumed. On one occasion the white and yolk of an egg beaten together were injected; but this was followed by an abscess. Each injection required from half an hour to an hour for its completion, and when the oil was injected slowly it gave rise to no pain. In this case it appears certain that the oil was absorbed and assimilated. (*New York Medical Journal*, March, 1876, from *Gazette Médicale de Paris*.)

The results of the experiments of Munzel and Perco, as well as the experience of Kreng and Whittaker, on the human subject, appear to be at variance with the experiments of Bernard and Barriswil; for while the latter found that crude albumen and gelatine were ejected from the blood without change when introduced into the veins, the former found their patients supported by these and other articles in the crude state when placed beneath the skin.

To reconcile this apparent discrepancy, it may be inferred that when crude articles in quantity are suddenly forced into the blood they are as suddenly forced out as foreign matter; but when these same articles are more slowly received into the blood vessels, as is the case in subcutaneous injections, the blood may have the power of digesting and fitting them for the assimilative process. There may be, therefore, such a thing as digestion in and by the blood.

FEEDING THROUGH THE SKIN.

It is believed by some that food may be introduced into the system by absorption through the skin. I have heard of children being bathed in beef tea under the impression that the tea was absorbed. It is probable that the water and organic salts are the only elements that enter the system.

This method of giving soups may, therefore, be condemned as comparatively useless. That oils can be admitted to the blood by friction I believe is a generally admitted fact. On many occasions I have applied cod-liver oil by friction to the skin of children suffering from marasmus, and have reason to believe with good effect. It is a matter of conjecture whether it enters the blood through the venous or lymphatic capillaries.

I have now imperfectly sketched the different methods of artificial alimentation, and have only further to say that two or more of these methods may be employed at the same time, if, in the opinion of the physician, the urgency of the case demands them.

Feeding by the nose and rectum, and through the œsophagus tube, must always be the chief artificial methods of administering food, since they are comparatively easy of performance and certain in results.

Besides the artificial methods of feeding above described, mention might be made of the transfusion of blood, and the intravenous injections of milk and peptones. As these subjects have recently occupied the attention of many eminent men, who are yet engaged in investigations, and as they do not strictly belong to the subject I have selected for the evening's paper, I will forbear giving the history and re-

* Read before the Bedford Medical Club. *N. Y. Medical Journal.*

sults of these operations. They are, however, of great importance, and deserve the attention of the profession. It is to be hoped that the investigations now being made will free them from the many errors and uncertainties by which they are surrounded, and place them on a sound and correct basis.

MORBID FEAR AS A SYMPTOM OF NERVOUS DISEASE.*

By GEORGE M. BEARD, M.D.

THE emotion of fear is normal to the human mind. It is as natural and as necessary to be afraid as to be courageous. Fear is, indeed, a part of the first law of nature, self-existence. The emotion is, therefore, physiological, varying both in degree and kind, with race, sex, age, and the individual. In neuropathology, especially in the pathology of functional nervous diseases, the difference between health and disease is of *degree* rather than of kind; the phenomena that belong to what we call health, passing by indefinite and not distinctly defined gradations into the phenomena of what we call disease; pathology being, in truth, as has been said, but the shady side of physiology.

Morbid fears are the result of various functional diseases of the nervous system, and imply a debility, a weakness, an incompetency and inadequacy, as compared with the normal state of the individual. A healthy man fears; but when he is functionally diseased in his nervous system he is liable to fear all the more; to have the normal, necessary fear of his physiological condition descend into an abnormal pathological state, simply from a lack of force in the disordered nervous system.

Thus it comes to pass that with the developments of functional nervous diseases in modern times, particularly with the increase of neurasthenia in its various phases, there has been an increase in the forms of morbid fears, and in the number of their manifestations. When any special phase of morbid fear assumes a considerable frequency and consistency, so as to allow of classification, it is proper and convenient to give it a special name by which it can be known, described, and referred to. With the understanding that these morbid fears are symptoms of diseases, rather than diseases of themselves, simply belonging to a large family of symptoms, it is a very important convenience to be able to recognize them, to interpret their meaning, to understand their relations to the other members of the same family of symptoms, and to be familiar with their diagnosis and treatment. It would probably be a correct statement to say that no symptom of functional nervous disease is so likely to be overlooked, or slighted, or misinterpreted, or improperly named, as this one symptom of morbid fear; it is diagnosticated as hysteria, hypochondria, dyspepsia, imagination, biliousness, and actual insanity. Insanity has, it is true, its morbid fears, but they are associated with delusions or hallucinations.

There are quite a number of varieties of morbid fear associated with cerebrasthenia, or brain exhaustion, without any hallucinations or delusions. The patient knows that there is no just, objective ground for his fear, but his emotional nature, under the influence of his exhausted nervous condition, overcomes his reason.

A number of years ago, I described a form of morbid fear under the term *astraphobia*, or *fear of lightning*, from the Greek *astrape* and *phobos*, fear. Of this disease I have seen quite a number of cases, and have nothing to say in regard to it beyond what has been already published.

The leading symptoms are headache, numbness and pain in the back of the head, nausea, vomiting, diarrhea, and, in some cases, convulsions. These symptoms are preceded and accompanied by great dread and fear. One of my patients tells me she is always watching the clouds in summer, fearing that a storm may come. She knows and says that this is absurd and ridiculous, but she declares she cannot help it. In this case the symptom was inherited from her grandmother; and even in her cradle, as she is informed by her mother, she suffered in the same way. A lady now under my care, the wife of a clergyman, was first attacked with these symptoms six years ago, in connection with other symptoms of general neurasthenic and uterine difficulties. Her husband tells me that on the approach of a thunder storm he is obliged to close the doors and windows, darken the room, and make things generally inconvenient for himself and family.

Westphal more recently has described a form of morbid fear under the term *agoraphobia*, or *fear of places*. This title, however, is quite inadequate to express the many varieties of morbid fear which the expression fear of places covers. The Greek word *agora*, from which Westphal derives his term, means an open square—a market place, a public place where assemblies were held—and as applied to the cases first described by him, the term is practically, though not etymologically, a correct one, for the fear of going across open squares or places, at a distance from houses or shops, was the chief feature in the cases described by him. This fear of open squares or places is, however, but one of a large number of phases that the fear of places assumes, as I have elsewhere described. In strictness, fear of places should be derived from the Greek word *topos*, place, a generic term, while *agora* is a special kind of place; *agoraphobia* would, therefore, be a species of *topophobia*, a general fear of places, which symptom seems to be capable of infinite variety. Thus one of my cases, a gentleman of middle life, could walk up Broadway without difficulty, because shops and stores, he said, offered him an opportunity of retreat, in case of peril. He could not, however, walk up Fifth avenue, where there are no stores, nor in side streets, unless they were very short. He could not pay a visit to the country in any direction, but was hopelessly shut up in the city during the hot weather. One time, in riding in the stage up Broadway, on turning into Madison square, he shrieked with terror, to the astonishment of the passengers. The man who possessed this interesting symptom, was tall, vigorous, full faced, and physically and mentally capable of endurance. He had, however, other symptoms of cerebrasthenia.

These fears take opposite phases; thus, with one it is impossible to go to a certain place, where he was perhaps first attacked with the evil symptoms. And another finds it impossible or very difficult to go out of his house to any distance where business calls. I have now under care a patient who for a long time has been shut up in his house, unable to go anywhere, simply from fear of going anywhere. For a long time he was unable to come to consult me; but now I see him regularly; but he did not, until lately, since he has improved, go anywhere else. Quite a number of persons I have seen who find it difficult to go on long journeys, and if they do go, must have company. A person wrote me from a distant city in the West, expressing a desire to come and consult me, but upon reaching a city at some distance was compelled to return home without reaching New York. All these forms of morbid fear—fear of leaving home, fear of going to any locality or in any direction, fear of travel—are properly varieties of *topophobia*, the fear of open squares or places being relegated, though not quite correctly, by *agoraphobia*.†

Dr. Meschede brought to the attention of the physicians at Cassels, in Germany, a form of morbid fear quite the opposite of what is known as agoraphobia, or fear of *open* places. In his case the symptom was fear of *close*, *narrow* places. The patient, a young man twenty years of age, was seized with a feeling of giddiness and confusion when in a small, narrow room. In the summer he could not sleep in a room at all, but was obliged to camp out; in winter he slept in a large, airy room. He was obliged to give up his studies and become a farmer. This symptom cannot be classed as agoraphobia at all, for it is the reverse condition. It belongs properly to what I call *topophobia*, fear of places; and is, like agoraphobia, a species of which topophobia is the genus.

A form of fear I have lately described is *anthropophobia*, derived from the Greek *anthropos*, man, and *phobos*, fear. This term applies to aversion to society, a fear of seeing, encountering, or mingling with a multitude, or of meeting any one besides ourselves. This phase of morbid fear has different varieties. One form is *gynephobia*, *fear of women*, from the Greek *gune*, woman, and *phobos*, fear.

Some patients afflicted with cerebrasthenia have no fear of male society, but are particularly timid at even the approach of females. They can mingle with men in ordinary business relations, but dread to go in any company where women are found, even when not particularly bashful. A person once consulted me for gynephobia which took on a peculiar form, he being only afraid of women in the society in which he moved; women of the lower order he cared nothing for, and he had no anthropophobia, or fear of man. In quite a number of cases this fear of man is so severe as to compel patients to give up business entirely; and I know a number of cases where men of strong muscles and having the appearance of great physical strength have been compelled through this symptom alone to withdraw from the occupations in which they were engaged; they could not face men, deal with them, persuade them to buy or sell, or have any influence over them; they dreaded to meet a human being. This form of morbid fear is often accompanied with turning away of the eyes and hanging down of the head, but not necessarily so, and usually so only in the severer cases. This phase of morbid fear is a very good barometer of the condition of the system. From this alone we can often judge whether the patient is improving or growing worse. It is a very interesting symptom. In some cases I hold the head of the patient between my hands, so as to bring his face opposite mine, and even then he will involuntarily turn away his eyes. This phase of morbid fear also has its opposite. In some persons there exists what may be called *monophobia*, or fear of being alone. Some of these persons cannot travel alone, but have no difficulty in traveling if they are in company with some one. Sometimes they cannot walk the street alone or leave the house except in company.

A form of morbid fear that has long been known to the profession is pathophobia, or fear of diseases—more commonly known as hypochondriasis. This form of morbid fear seldom exists alone, but is found in company with other symptoms—some real disorder of the nervous system. The pathophobic sufferer, with brain or stomach, or both, exhausted for some reason, may fear disease of the heart, of the stomach, or of the brain, or of the reproductive system, even when there is no sign of disease except his fear. The mistake usually made in the study of these cases is to assume that this fear of disease is the only symptom which the patient has, and that it is the cause of the disease; whereas, usually, it is the result of the disease, whatever the cause may be; and as such should be studied and treated.

There is a manifestation of morbid fear which is not uncommon, and to which we might perhaps give the term *pantophobia*, or fear of everything; all responsibility, every attempt to make a change of movement being the result of dread and alarm. The wife of one of my patients has a morbid fear in reference to one of her sons, a lad of about fifteen years of age; and so distressed is she by it that she cannot allow him to go out of the house or out of her sight, fearing lest he may be kidnaped, or some harm may come to him, as in the case of Charley Ross. The poor fellow is thus kept a prisoner most of the time, and the whole family is disturbed and annoyed. He must remain in the city during the summer, as she cannot allow him to leave town; and at no season can he go anywhere unless accompanied by his tutor.

A lady now under my treatment who is also *astraphobic*, tells me that she is afraid to go into the street, to do any shopping, or attend to any business; that it is an affliction for her to come to see a physician; everything is a dread to her, even when there is no draught made upon her physical strength.

The expression *phobophobia*—fear of fears—might possibly apply to a certain class of nervous patients, who fear they may fear, provided they make an attempt to move or go in any direction where their mobid fear is in the way; they are afraid even when they do and say nothing. These persons fear when they are entirely still and inactive, from a fear that if they attempt to do anything they will be attacked with their especial morbid fear. One of my patients—a stout, and large man—in addition to topophobia (fear of places) had at one time a fear of committing some crime against women that would disgrace him. He was ashamed of his fear, he could not help it, although he has now entirely recovered.

Mysophobia, fear of contamination, lately described by Dr. Hammond, comes under this head; the results of the treatment showing very clearly that it is symptomatic of a similar or analogous condition of the brain. In those cases there were no hallucinations or delusions.

In regard to all these different forms of morbid fear, by whatever name they are known or described, these general propositions are true and verifiable.

First.—These morbid fears are symptomatic of functional, never or rarely of organic diseases. The existence of any of these symptoms in a doubtful case of diagnosis, would alone almost establish the nature of the disease, or enable us to give the casting vote.

The best test of skill in the practice of neurology is in making a differential diagnosis between functional and organic diseases in their early stages; for this cause alone morbid fears demand close attention.

While it is possible for hysterical and neurasthenic symptoms to appear and maintain themselves, more or less, in organic diseases, yet these symptoms of morbid fear are not found, according to my observation, in what we call organic or structural diseases of the brain or spinal cord; it is strange that they are not, but the fact as here related is verifiable.

They are not found in insanity itself, and the habit of calling them forms of mania or delusion is not based on fact or a right study of these cases. I observe that even now some forms of morbid fears are classed under insanity, or mania of some kind, even when there are no delusions or hallucinations. When the insane have morbid fears such as I have described, or very many others which they may have, and do have, as we all know, they are delusions out of which they cannot be reasoned, and are a part of, and in harmony with other delusions of the insane. But in all the cases to which I have here referred, there are no hallucinations whatever; the patient is as well aware of his delusion as his friends are, and is as anxious to get rid of them as he would be of a sick headache, fever, or paralysis; but he is unable to shake them off until his exhausted brain, of which they are the direct result, is strengthened by hygiene and time treatment.

Second.—These symptoms may come on suddenly, in some cases almost instantaneously, and when once they appear, they may exist for months and years, varying in intensity at different times, like other symptoms of cerebrasthenia, with which they are often associated.

Third.—These morbid fears are very frequently, though not always or necessarily, the result in whole or part of disorder of the reproductive system.

Excess in the male in the natural or unnatural ways, or prolonged and teasing continence united with sexual excitation, and, in the female, various slight and superficial uterine erosions, or displacements or lacerations, are the common provoking uses of these morbid fears, especially in constitutions where the nervous diathesis predominates.

These fears may exist long after the local difficulty has been cured; in this respect these symptoms follow the law of the nervous symptoms with which they are so often associated. Some of these cases are anæmic, but the majority are not so, and many are models of physical strength.

Fourth.—These morbid fears rarely exist alone. They almost always appear in connection with other symptoms of neurasthenia, either myelasthenia, exhaustion of the spine, or cerebrasthenia, exhaustion of the brain, most frequently the latter. I think, indeed, that I have never seen a case of morbid fear, such as I have here described, that existed alone, without some one accompanying neurasthenic symptom, or many such symptoms. In some cases, I admit, these accompanying symptoms are few and slight, and can be ascertained only by careful study.

Among those associated symptoms may be mentioned palmar-hyperidrosis, flushing of the face, a feeling of profound exhaustion, insomnia, hopelessness, shooting pains in the extremities, excess of oxalates and urates in the urine, heaviness of the loins and limbs, dilated pupils, local spasms of muscles. Only rarely, however, is there a complete picture in which all these symptoms are represented. Like all these symptoms of neurasthenia morbid fears very often occur in those of great, even enormous muscular strength and endurance; many of them can walk and work all day with muscle and with brain, but in the presence of their special fears they are as infants.

A very frequent accompanying symptom is dizziness. Many of these cases, when they approach the object of dread, or even think of approaching it, are seized with vertigo—sometimes with less defined abnormal sensations. I have seen three cases where an epigastric spasm appears on attempting or even thinking of doing anything which is a dread. I have now under care a patient who tells me that he has a spasm in the stomach whenever he thinks of doing anything where he fears a failure. He describes it as a sudden sinking, a falling, somewhere between the base of the lungs and the navel.

This patient has also a large array of correlated nervous symptoms, such as sweating of the hands, twitching of the eyelids, mental depression, etc. One of these cases had this symptom of spasm—sinking in of the stomach—while at school, and it would come upon him whenever he was called upon, or feared he might be called upon, to recite; even the thought of responsibility, though it might be in the remote future, brought on the attack. The very existence of a morbid fear suggests to us that we search for other symptoms.

Fifth.—The treatment of morbid fear is the treatment of the condition of the brain, of which it is a symptom of the local or general condition on which the brain exhaustion depends; very generally stated, this condition requires both constitutional and local treatment. The constitutional treatment includes the whole array of sedatives and tonics, the more effective being the bromides and electricity; and counter-irritation at the back of the neck and in the bowels by means of cathartics. The local treatment in cases of disorder of the prostatic urethra in males consists in my own practice of the following procedures: very mild electrolysis with the urethral electrode—application of liquor bisnuthi—of iodoform, by suppositories, by sounds, and *dry* cold in the urethra and the rectum.

These cases can be cured, and be permanently cured, but cannot be cured suddenly nor usually by a single prescription. They have been sick before we see them for months and oftentimes for years. The details of the treatment must be varied with the idiosyncrasy of the patient. The causes of failure are three-fold. First, the exclusive use of general treatment by medication, the local irritation from which the symptoms start being undetected. Secondly, the use of stimulants where sedative treatment is required. Thirdly, the want of change in the modes and details of treatment, and perseverance id their use.—*The Hospital Gazette.*

* Read at the annual meeting of the American Neurological Association, June 20th, 1879.

† In etymological strictness agoraphobia means fear of large assemblies of human beings, and not of the place where the people meet.

MAMMARY INFLAMMATION TREATED BY ICE.

A WRITER in the *British Medical Journal* says that the suggestion of treating threatened inflammation of the mammary gland by ice was "one of the most valuable hints he ever got." He thus describes its use in a case:

"I used a large Chapman's spine-bag, filled with ice, which encircled the lower half of the breast. It felt very cold indeed for a minute or two, then a considerable quantity of milk was shot out, as from a syringe (no milk had flowed before), the pain abated, and in an hour was almost gone; I now renewed the ice in the bag, and the patient kept it closely applied with her arm, which was protected from the cold by a folded towel. Next morning, I found her hugging the ice-bag and loud in its praise. She continued suckling her infant; but she suggested that the baby should not be put to the breast oftener than two or three times in the twenty-four hours. On the fourth day after the commence-

ment of the ice the most careful examination failed to detect anything wrong in the breast, and she is now quite well and nursing her child. No other remedies were used."

THE SECRETION OF THE GASTRIC GLANDS.

PROFESSOR HEIDENHAIN succeeded in separating a considerable portion of the fundus of the stomach in a dog from its connection with the rest of the organ, and forming it into a blind sac communicating with the exterior of the body. This enabled him to collect the secretion of the gastric glands unmixed with that of the pyloric glands, and uncontaminated by the saliva and other liquids which pass down the œsophagus. The secretion is a clear, strongly acid liquid, containing an unexpectedly small amount of mucus, and an average of 0·45 per cent. of solid matter, partly organic, partly inorganic, the former consisting mainly of pepsine. The average acidity of the liquid is equivalent to 0·52 per cent. of hydrochloric acid, which is far higher than that of the mixed gastric juice, free from saliva, examined by Bidder and Schmidt. Richet, from observations on the juice of a man with a gastric fistula, found that when fresh it contained only hydrochloric acid, while when kept for a time it developed an organic acid, probably sarcolactic. No such acid was observed to be produced in the secretion obtained from the dog. It was found that the introduction of nutritious food into the stomach induced active secretion in from fifteen to thirty minutes, and this continued until the stomach had completely emptied itself. But if indigestible substances were introduced no secretion flowed from the sac for upwards of an hour. Water was then given to the animal, and secretion commenced, but only lasted an hour and a half.

From these and other experiments, Professor Heidenhain concludes that mechanical stimulation of the stomach excites secretion only at the point of contact, general activity of the grandular apparatus requiring absorption for its production. If the composition of the secret liquid be examined at regular intervals during the digestive process, its acidity is found to remain pretty uniform, but the proportion of pepsine contained in it undergoes a peculiar and orderly series of variations. During the second hour it sinks rapidly to a minimum; towards the fourth or fifth hour it rises again to a point generally higher than at first, and remains at or near this point for a considerable time. These variations are quite independent of the amount of pepsine actually contained in the glands, which is known to sink steadily. The secreting surface can pour out a liquid very rich in pepsine at a time when its poverty in this substance is most strongly marked. No definite conclusion can at present be arrived at as to the cause of this phenomenon.—*Pflüger's Archiv*i *Academy*.

MELTING POINTS OF THE ELEMENTS AND THEIR COEFFICIENTS OF EXPANSION BY HEAT.

By T. CARNELLEY.

A TABLE which the author has compiled shows that with five exceptions the coefficient of expansion increases as the melting point sinks. The five exceptions are arsenic, antimony, bismuth, tellurium, and tin; the three former of which belong to the same elementary group, and even these among themselves display a similar relation between their melting points and coefficients of expansion. Why these five bodies form an exception does not yet appear. It must, however, be noted that they are all found on the ascending side of Meyer's Curve of the Elements (see his "Modern Theories of Chemistry"), whilst three of them, tin, antimony, and tellurium, follow immediately upon each other in the above curve. Arsenic, antimony, bismuth, and tellurium are all very brittle and belong to the same crystalline system; and bismuth and antimony are the only two known pure elementary bodies which expand on congealing. Tin, in some of its compounds, displays an abnormal melting point, as will be shown in a future memoir. Both the melting points and the coefficients of expansion may be periodic functions of the atomic weight.

PURIFICATION OF PLATINUM AND IRIDIUM.

AN alloy of these metals is used in the manufacture of the standard weights and measures. In a paper recently communicated to the Royal Society, Mr. G. Matthey describes the methods he employed for preparing the metals in a state of purity.

The following is an abstract:

The six metals (of which platinum is the chief) usually found more or less in association, present characteristics of interest beyond their metallurgical utility, which are, perhaps, worth alluding to. It is, for instance, a curious fact that the group should consist of three light and three heavy metals, each division being of approximately the same specific gravity—the heavier having (in round figures) just double the density of the lighter series.

Thus we find osmium, iridium, platinum, forming the first division, of the respective specific gravities of 22·43, 22·39, 21·46; whilst ruthenium, rhodium, and palladium are represented by the figures 11·40, 11·36, 11, the average densities of the heavy and light divisions thus being respectively 22·43 and 11·25.

But a more interesting and important classification is what I may designate as a first and second class series, from the more important view of their relative properties of stability. Thus platinum, palladium, and rhodium form the first or higher class, not being volatilizable in a state of oxide; iridium, osmium, and ruthenium forming the second or lower class, their oxides being more or less readily volatilized.

The oxide of iridium is affected at 700° to 800° C., and entirely decomposed at 1,000°, whilst osmic and hyporuthenic acids are volatilized at the low degree of 100°, the latter exploding at 108°. The chlorides of these metals can be sublimed at different temperatures (as also the protochloride of platinum).

PLATINUM.

The preparation of this metal in a state of purity is an operation of extreme delicacy. I commence by taking ordinary commercial platinum; I melt this with six times its weight of lead of ascertained purity, and after granulation, dissolve slowly in nitric acid diluted in the proportion of 1 volume to 8 of distilled water. The more readily to insure dissolution, it is well to place the granulated alloy in porcelain baskets such as are used in the manufacture of chlorine gas for holding the oxide of manganese. When the first charge of acid is sufficiently saturated, a fresh quantity should be added until no more action is apparent; at this stage the greater part of the lead will have been dissolved out, together with a portion of any copper, iron, palladium, or rhodium that may have been present. These metals are subsequently extracted from the mother-liquors, the nitrate of lead by crystallization, and the remaining metals by well known methods.

The metallic residue now obtained will be found in the state of an amorphous black powder (a form most suitable for further treatment), consisting of platinum, lead, and small proportions of the other metals originally present—the iridium existing as a brilliant crystalline substance insoluble in nitric acid. After digesting this compound in weak aqua regia, an immediate dissolution takes place of the platinum and lead, leaving the iridium still impure, but effecting a complete separation of the platinum.

To the chloride of platinum and lead after evaporation is added sufficient sulphuric acid to effect the precipitation of the whole of the lead as a sulphate, and the chloride of platinum, after dissolution in distilled water, is treated with an excess of chloride of ammonium and sodium, the excess being necessary in order that the precipitated yellow double salt may remain in a saturated solution of the precipitant; the whole is then heated to about 80°, and allowed to stand for some days; the ammonio-chloride of platinum will settle down as a firm deposit at the bottom of the vessel, whilst if any rhodium, as is generally the case, is present, the surface liquor will be colored a rose tint, occasioned by a combination of the salts of the two metals.

The precipitate must be repeatedly washed with a saturated solution of chloride of ammonium and subsequently with distilled water charged with pure hydrochloric acid. This is necessary for its purification. The small quantity of the double salt which will be taken up and held in solution is of course recovered afterwards. Rhodium may still exist in the washed precipitate, which must therefore not be reduced to the metallic state until its separation is completed, and this is the best effected by mixing with the dried compound salts of chloro-platinate and chloro-rhodiate of ammonia, bi-sulphate of potash with a small proportion of bi-sulphate of ammonia, and subjecting to a gradual heat brought by degrees up to a dull red in a platinum capsule, over which is placed an inverted glass funnel. The platinum is thus slowly reduced to a black spongy porous condition freed from water, nitrogen, sulphate of ammonia, and hydrochloric acid, the rhodium remaining in a soluble state as bi-sulphate of rhodium and potash, which can be dissolved out completely by digesting in boiling distilled water; a small quantity of platinum will have been taken up in the state of sulphate, but is regained by heating the residue (obtained on evaporation) to redness, which reduces it to the metallic condition, the rhodium salt remaining decomposed.

By the method above described the platinum is freed not only from rhodium, but from all other metals with which it may have been contaminated, and is brought to a state of absolute purity, of the density 21·46, the highest degree obtainable.

IRIDIUM.

In practice, the purest iridium which can be obtained from its ordinary solution (deprived of osmium by long boiling in aqua regia and precipitated by chloride of ammonium) will almost invariably contain traces of platinum, rhodium, ruthenium, and iron.

I fuse such iridium in a fine state of division with ten times its weight of lead, keeping it in a molten state for some hours, dissolve out the lead with nitric acid, subject the residue to a prolonged digestion in aqua regia, and obtain a crystalline mass composed of iridium, rhodium, ruthenium, and iron, in a condition suitable for my further treatment. By fusion at a high temperature with an admixture of bi-sulphate of potash, the rhodium is almost entirely removed, any remaining trace being taken up together with the iron in a later operation. The iridium so far prepared is melted with ten times its weight of dry caustic potash, and three times its weight of niter, in a gold pan or crucible; the process being prolonged for a considerable time to effect the complete transformation of the material into iridiate and ruthenate of potash, and the oxidation of the iron; when cold, the mixture is treated with cold distilled water. The iridiate of potash of a blue tinge will remain as a deposit almost insoluble in water, more especially if slightly alkaline, and also the oxide of iron.

This precipitate must be well washed with water charged with a little potash and hypochlorite of soda until the washings are no longer colored, and then several times with distilled water.

The blue powder is then mixed with water strongly charged with hypochlorite of soda, and allowed to remain for a time cold, then warmed in a distilling vessel, and finally brought up to boiling point until the distillate no longer colors red, weak alcohol acidulated with hydrochloric acid.

The residue is again heated with niter and potash water charged with hypochlorite of soda and chlorine, until the last trace of ruthenium has disappeared.

Further, to carry out the purification, the blue powder (oxide of iridium) is redissolved in aqua regia, evaporated to dryness, redissolved in water, and filtered.

The dark colored solution thus obtained is slowly poured into a concentrated solution of soda and mixed with hypochlorite of soda, and should remain as clear solution without any perceptible precipitate, and subjected in a distilling apparatus to a stream of chlorine gas, should not show a trace of ruthenium when hydrochloric acid and alcohol are introduced into the receiver. In this operation the chlorine precipitates the greater part of the iridium in a state of blue oxide, which, after being collected, washed, and dried, is placed in a porcelain or glass tube, and subjected to the combined action of oxide of carbon and carbonic acid obtained by means of a mixture of oxalic with sulphuric acid gently heated.

The oxide of iridium is reduced by the action of the gas leaving the oxide of iron intact; the mass is then heated to redness with bi-sulphate of potash (which will take up the iron and any remaining trace of rhodium), and after subjecting it to many washings with distilled water, the residue is washed with chlorine water to remove any trace of gold, and finally with hydrofluoric acid, in order to take out any silica which might have been accidentally introduced with the alkalies employed or have come off the vessels used.

The iridium, after calcination at a strong heat in a charcoal crucible, is melted into an ingot.

ALLOY OF IRIDIO-PLATINUM.

Operating upon a charge of 450 ounces of platinum and 55 ounces of iridium, I commenced by melting these metals together and casting into an ingot of suitable shape, which I then cut into small pieces with hydraulic machinery. After remelting and retaining in a molten condition under a powerful flame of oxygen and common gas for a considerable time, I recast and forged the mass at an intense white heat under a steam hammer, the highly polished surfaces of which were cleaned and polished after each series of blows—when sufficiently reduced the alloy was passed through bright polished steel rollers, cut into narrow strips, and again slowly melted in a properly shaped mould, in which it was allowed to cool. I thus obtained a mass of suitable shape for forging, perfectly solid, homogeneous, free from fissures or air holes, and with a bright and clean surface.

A piece cut from the end of a mass so prepared was presented to the French Academy of Science, and gave the following results:

Weight in air...	116·898	grms.
" water..................	111·469	"
Showing a density of....	21·516	"

thus proving that the necessary processes of annealing at a high temperature had caused it to resume its original density.

The analysis gave—

	1	2
Platinum......................	89·40	89·42
Iridium	10·16	10·22
Rhodium	0·18	0·16
Ruthenium.....................	0 10	0.10
Iron..........................	0·06	0 06
	99·90	99·96

From which is deduced—

	Proportion.	Density at zero.	Volume.
Iridio-platinum, at 10 per cent...	99·33	21 575	4·603
Iridium, in excess..............	0·23	22·380	0 010
Rhodium.................	0·18	12·000	0 015
Ruthenium.....................	0 10	12·261	0·008
Iron....	0·06	7·700	0·008
	99·90		4·644

Density at zero, calculated after No. 1 analysis, 21·510
Density at zero, calculated after No. 2 " 21·515

which coincide perfectly with the practical results obtained."

MM. Deville and Mascart find the coefficient of dilatation to be from 0° to 16° C. 0·00002541.

ON ULTRAMARINE.

By M. T. MOREL.

THE qualities which are commercially required in ultramarines are a deep and brilliant shade, fineness, coloring power, resistance to acids, and resistance to alum. All the trials made on this subject are simply comparative, and refer to a sample which has already been chosen as a standard.

Shade.—The two samples are placed side by side upon white paper, by means of a spatula (at a *north* window). If one of the samples is darker than the other, this is best ascertained by crushing a small quantity of this upon the other.

Fineness can scarcely be recognized except by feel. Only two other methods are known which may be used, and both are apt to lead to inaccurate results.

It has been proposed to stir up the ultramarine in water, and note the time which it takes to settle. But this time varies for one and the same ultramarine, according to the manner of manipulation, and often a very fine ultramarine settles more rapidly than a coarse one of another make.

Ultramarines may also be diluted with white powders, comparing the intensity of the blues thus let down. This method does not indicate absolutely the fineness, but is, properly speaking, a measure of the coloring power. We may draw from it useful indications; thus, in general, the finer an ultramarine the more it retains its blue shade when diluted with white. This observation is applicable only when the ultramarines do not differ too much in tone.

Coloring power is best recognized by mixing with a certain quantity of a white powder, the finer the better, such as kaolin. One part of the blue is mixed with six parts of the white, and the ultramarine thus let down is compared with the standard sample, which has been treated in the same manner. To have results exactly comparable, it is necessary to do all the weighings at once, as both ultramarine and kaolin absorb moisture from the air. The tints obtained may be either of a pure blue, or of a greenish, a violet, or a rose blue, according to the nature of the sample. Thus the comparison of the different kinds is rendered very difficult. (What is the difference between a violet blue and a reddish or rose blue?)

Resistance to acids is a necessary quality for ultramarines, which have in their use to come in contact with liquids which are either acid or capable of becoming so. This occurs in the pigment style of printing, where the albumen and other materials which serve to fix ultramarine upon the fiber are rapidly decomposed and turn acid. To come as close as possible to the actual condition of the case, the trial is made with a solution of oxalic acid, which is one of the products of the decomposition of albumen. A standard solution is made by dissolving 1¾ oz. of crystalline oxalic acid in 35 fluid ounces of water.

The one-hundredth part by measure of this solution is put into a test tube along with 7·7 grains of the ultramarine, and is well shaken up, while the change which ensues is compared with what takes place in a portion of the standard ultramarine similarly treated.

Resistance to Alum.—Formerly for ultramarine used in the paper manufacture resistance to alum was the first consideration. Now it is found that, if the paper is manufactured under right conditions, the most delicate ultramarines are not affected.

Still, if it is desired to measure this resistance, we make a saturated solution of alum, and pour 1½-hundredth parts of 35 fluid oz. thereof into a test tube, along with 7·7 grains of the sample under examination, making, of course, a comparative experiment with the standard sample. It is necessary to shake up frequently, lest the ultramarine should form a clot.

It is generally believed that an ultramarine which resists alum will resist acids equally well. This is an error; the resistance to alum is quite different from the resistance to acids.

In the paper trade coloring power is the first condition required. The greatest possible fineness is needful to avoid blurs. If much alum is used the resistance of the sample to alum should be tried.

In printing fineness is a main point, especially for dark-

that we sometimes find eight instead of six calyx lobes. As regards the "two forms," nine of the fifteen are without glandular heads, and these are arranged in precisely the same manner as are the true stamens of the male flowers both of sassafras and of lindera. The remaining six have the anther-like heads, and are arranged in pairs like those of the male flowers.

Now, it is not probably doubted by any one that at one time the flowers of these two genera were perfect, *i.e.*, hermaphrodite, and this sufficiently accounts for the obsolete stamens of the female flowers of sassafras, but it does not explain why there are only six of these when the male flower has nine perfect stamens. Neither does it supply a reason for the six very similar glands of the male flower in addition to the true stamens. Still less does it account for the nine glandless and six gland-bearing rudiments in the female flowers of lindera.

In view of all the facts I, therefore, conclude:

1. That the six glands in the staminate flowers of both genera are the true homologues of the six rudimentary stamens in the fertile flowers of sassafras, and of the six gland-bearing rudiments of stamens in the fertile flowers of lindera.

2. That the nine polliniferous stamens of the male flowers of both genera are transformed petals of a former stage, in which the flowers were dichlamydous.

3. That the nine glandless filaments in the female flowers of lindera are the true homologues of the present stamens of the male flowers, which may or may not have once possessed anthers also.

4. That at the time when the present stamens were petals the present gland-bearing rudiments were the true stamens.

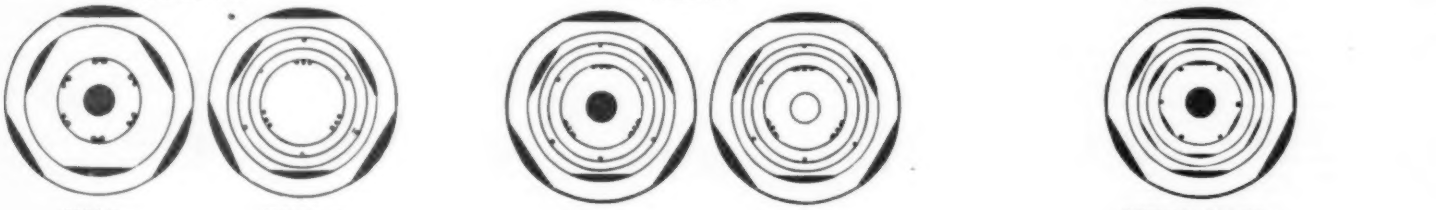
The flowers of the *Lauraceæ* would therefore seem to have passed through a two-fold transmutation, having their petals first transformed into stamens, at least in one (the male) form, and then gradually losing their original stamens by their conversion into rudimentary glands. These not being injurious, but rather affording some protection, have not disappeared altogether, but remain very conspicuous objects. It is still a question whether to regard the nine glandless filaments in the female flowers of lindera as abortive stamens or as the remains of the original petals; in other words, whether the petals in the female flowers were ever really transformed into stamens, or whether they simply disappeared in sassafras and are now disappearing in lindera. I am disposed to adopt the latter view.

The peculiar connective joining the rudimentary stamens with the inner row of true stamens and of rudimentary petals in the female flowers of lindera may be explained by supposing that the original six stamens were situated one on each side of the three inner petals and inserted with them on the tube of the calyx.

The two genera which I have described, therefore, and perhaps all the plants of the laurel family, must be supposed to have descended from a common progenitor in the remote past, which differed in some important respects from any plants now existing. This primordial ancestor of the Lauraceæ possessed dichlamydous and eleutheropetalous, hermaphrodite flowers, with a six parted calyx, the divisions arranged as now, nine narrow petals inserted in three rows on the calyx tube, the outer and inner rows opposite the outer lobes, and the intermediate row opposite the inner lobes; and six stamens inserted at the base of the inner petals, one on each side.

The following diagrams, representing cross sections of each of the forms now found, and also of the original from which all are supposed to have been derived, will serve to illustrate the progress of the changes which have taken place. The inner circle represents the gynæcium, which is not drawn when wanting, and not filled out when abortive. The next three whorls embrace the present andræcium, as also the primordial petals. In the female of sassafras only one of these (the innermost) occurs representing the primordial andræcium, now the anther-like glands. The two outermost whorls denote the lobes of the calyx. In the ancestral form the nine petals are represented as somewhat ligulate, a stamen on each side of each of the three inner ones.

SASSAFRAS LINDERA PROTOTYPE



MALE FEMALE FEMALE MALE HERMAPHRODITE

At first view it may be thought that this is an extreme and complicated metamorphosis for a flower to undergo, but it must be remembered that a vast period of time has been required to effect it. We know as well as we know anything about plants, that the flowers of many of them are actually in a transition state. The number of species that any botanist can call to his mind in which the sexes are only partially separated and apparently in a vacillating condition between hermaphrodism and diclinism is very great. They occur in families the most widely separated botanically, and illustrate all shades of the process. Even lindera, which we have been studying, differs from its close relative, sassafras, in this respect. The rudimentary pistil in the male, and the rudimentary petals (or stamens) in the female flowers, both of which have long since disappeared from sassafras, plainly show that the latter is, so to speak, a much older genus, and commenced its career of transmutation ages further back in geologic time; and we may also with some certainty predict that lindera, too, will one day lose these useless vestiges. Nor are we restricted for time in the case of the *Lauraceæ*. Vegetable palæontology teaches that this is one of the oldest families of flowering plants. Its fossil remains are found deep down in Mesolithic strata. Prof. Lesquereux detects no less than seven genera of this order in the Dakota group of our American cretaceous formation. Among these occur a number of species of sassafras, but the absence of lindera is quite significant in view of its morphology, as above pointed out, indicating a more recent origin. These plants have therefore had, at least, the entire cretaceous, tertiary, post tertiary, and modern periods in which to undergo these changes, so that we only need assume the same measure of differentiation which we are certain is now taking place, to account for any modification which their present morphology may require to have taken place in the past.

INFLUENCE OF ELECTRICITY ON COLLIDING WATER DROPS.

LORD RAYLEIGH communicates a paper to the Royal Society on this subject. It has been long known that electricity has great influence on fine jets of water ascending in a nearly vertical direction. In its normal state a jet resolves itself into drops, which even before passing the summit of the column, and still more after passing it, are scattered to a considerable width. When a feebly electrified body is brought into the neighborhood of the jet, it undergoes a remarkable transformation, and appears to become coherent; under more powerful electrical action the scattering becomes more marked than at first. The latter action is due to mutual repulsion of the drops; the former has been hitherto unexplained. The cohesion seems to be more apparent than real; the seat of sensitiveness is at the place of resolution into drops; each drop carries away with it an electric charge, which can be collected in an insulated receiver. He is able to show that the normal scattering is due to the rebound of the colliding drops; such collisions being inevitable in consequence of the different velocities acquired by the drops under the action of capillary force, as they break away irregularly from the continuous portion of the jet. When the resolution is regularized by the action of external vibrations, as in Savart's and Plateau's experiments, the drops must still come into contact as they reach the summit of their parabolic path. Under moderate electrical influence, instead of rebounding after collision, they coalesce, and the jet is not scattered. This behavior of the drops becomes apparent under instantaneous illumination, such as that of an induction coil, into the secondary circuit of which a Leyden jar is introduced. To obtain further evidence two similar jets were made to collide horizontally, one being in communication with the earth, the other supplied from an insulated cistern. The sensitiveness to electricity was extraordinary. A piece of rubbed gutta percha brought near the insulated bottle at once determined coalescence. It was also possible to cause the jets again to rebound from one another, and then to coalesce.

Besides statical electricity, the electro-motive force of a single Grove cell was sufficient to produce the same phenomena, one pole being connected with the water, the other to earth. Even the discharge of a condenser charged by a single Grove cell answered the purpose. The writer indicates in conclusion the probable application to meteorology of the facts mentioned. The formation of rain must obviously depend materially on the consequences of encounters between cloud-particles. If the contacts result in coalescence the drops must rapidly increase in size and be precipitated as rain. We may thus anticipate an explanation of the remarkable but hitherto mysterious connection between rain and electrical manifestations.

ROTATION OF THE PLANE OF POLARIZATION BY ELECTRO-MAGNETISM IN A VAPOR.

MM. KUNDT and Rontgen have communicated their results on this subject to the Munich Academy. They have proved the fact of such rotation, which Faraday failed in demonstrating, at least in the vapor of sulphide of carbon. This substance was chosen for experiment because it shows strong electro-magnetic rotation in the liquid state, and because its vapor has great tension, even at moderate temperatures. An iron tube, closed at the ends by thick glass plates, was inclosed in an outer tin tube through which steam at 100° Cent. could be passed. The outer tube was surrounded by six large coils of wire, containing 400 turns of thick wire each, through which a current from 64 large Bunsen elements was passed. Sulphide of carbon vapor was heated as above described, and at the temperature of boiling water the vapor became transparent. A beam of polarized light was passed through the tube, and analyzed by Nicol's prism. On the current being sent, a distinct brightening of the field was observed, which increased, if, after rotating the prism afresh to darkness, the current was reversed. The rotation was in the direction of the positive current through the coils, and amounted to about half a degree. Sulphuric ether similarly treated gave no result. The authors of the paper are making apparatus by means of which to examine permanent gases and unsaturated vapors, with the special view of ascertaining whether oxygen rotates the plane of polarization in the same direction as other gases. Monsieur E. Bichat contributes a memoir to the *Comptes Rendus* of the Academy, in which he states that he has been following out a similar series of experiments since July, 1878, which give very different results from those noted above. He points out that the iron tube used by the German physicists very materially injures the completeness of the action. It is in reality a hollow electro-magnet; whereas his tube being of brass allows the whole force of the current to expend itself on the contained vapor. In illustration of the difference, he passes a tube containing liquid bisulphide of carbon through the two poles of an electro-magnet, obtaining a rotation of 10° 30′; but on using a single hollow core with the same current, no appreciable rotation is obtained. If the iron tube be thin, the action is not entirely annulled, but is considerably diminished—in the instance he gives from 5° to 1°. With his apparatus he has followed from zero to boiling point the rotation of the same current in sulphide of carbon and bichloride of tin. The molecular rotatory power is maintained as long as the boiling point is not approached. At that moment there is a diminution much more rapid than could have been foreseen from calculations based on the ratios of density.

JAMIN'S ELECTRIC LAMP.

THE annexed engraving is a representation of Jamin's electric lamp. It consists of two nearly parallel carbon rods, surrounded by an elliptical coil of wire, through which the current passes. It is this arrangement of the conductor in the shape of a coil around the rods that forms the novel part of the invention, and gives rise to the phenomena witnessed. The coil, being in the same vertical plane as the carbon rods, is arranged so that the current through it is in the same direction as that which flows through the latter, producing the arc at their extremities. In virtue of the law that currents in the same direction attract and those in opposite directions repel each other, the currents through the upper portion of the coil will attract the arc, and those through the lower portion will repel it. The lateral currents also, by reason of their tendency to deflect the arc into parallel-

ism, aid in repelling the latter to the extremities of the carbons. So powerful is this effect of repulsion that, if the number of turns of wire in the coil be too great, the arc, if caused to pass between the lower portions of the carbon rods, will move upward with great velocity, and the light becomes extinguished, owing to the arc being too strongly attracted in the direction of the extremities of the carbons. With this apparatus the arc becomes strongly curved; and it is stated that the light evolved is very considerably augmented by its use, owing to the carbons being no longer consumed laterally, so as to shade the light. By using this apparatus, also, the lamp may be inverted, without any danger of the arc quitting the extreme ends of the carbon rods.—*Eng. Mechanic.*

AN OPTICAL ILLUSION.

MR. R. A. PROCTOR writes in the *Newcastle Chronicle:*

Form a tube of card or paper some nine or ten inches long and an inch or so in diameter. Look through this at any object with either eye, holding a hand in front of the other eye close by the farther end of the tube, in such a way that the middle of the hand hides from the view of that eye the object seen through the tube with the other. Then, when both eyes are open, you seem to see the object through a round hole in your own hand; and looking scrutinizingly at this singular opening you perceive that it is lined with card or paper. I am not sure that the following explanation is much more than a restatement of the fact in a somewhat roundabout way—yet it does, perhaps, present the *rationale* of the matter as clearly as can be done in default of any satisfactory explanation of vision itself. (For, after all, it must be remembered that we can no more explain the form of sensation which we call vision than we can explain the form of attraction which we call gravity.)

We can recognize the nature of vision, and we can perceive how the law of optics follow, just as we recognize the attraction of gravity and perceive thence how the laws of planetary motion follow. But we cannot tell why the law of gravity exists, and we are equally in the dark as to the true significance of vision. We see with both eyes but one field of view. Ordinarily, this field includes, without confusion, what is seen by both eyes; there is not any confusion ordinarily even when the two pictures are perfectly distinct, as when the range of vision includes near objects and distant ones (for then, as any one will find who shuts alternately the right and left eye, the arrangement of the objects seen with either eye is entirely different, and actually different objects are visible with one and the other eye). But this power of adaptation is not unlimited, and the experiment described by Mr. Barkas, like the one I have described, taxes the power unduly. The real wonder is, *not* that the results of such experiments are so strange, but that results almost as strange are not constantly resulting from binocular vision.

The single field of view *must* include what is seen with one eye—namely, a small bright picture, limited by the dark (because shadowed) inclosure of the tubing. The field of view must also include what is clearly seen with the other eye, that is in the art gallery experiment the plank's surface, and, in the one I have described, the hand. Necessarily then the small bright image is seen on a background of plank in one case, and of hand-surface in the other, and, being obviously remote, seems to be seen through the plank, or through the hand. That the tint of plank, or hand, is entirely lost over the small field of view really belonging to the eye which looks through the tube, can be understood when we remember that, as this eye is shielded from the light, the pupil probably dilates, and apart from the inherent luster of the object seen in this way, the eye, owing to this change, sees it as a brighter object. It is indeed noteworthy that as a rule the mind, in dealing with optical impressions, refuses to combine two impressions belonging to the same part of the field of vision, even though both eyes may help to convey them. Thus, as Ruskin has pointed out, I think, if we notice the images of clouds in clear water, we do not at the same time see the stones and other objects at the bottom of the water; if we note these, we do not see the clouds. I may remark that in talking about binocular vision (though I believe I have here reasoned correctly), I am somewhat like blind Prof. Sanderson discussing the laws of optics. For the difference between the focal lengths of my two eyes is so great that I have never seen any object with both eyes at once, and probably I never shall. If I bring the two eyes to the same focal length, by using a very strong concave glass for my very short-sighted left eye, leaving the other eye unaided, I simply see all objects double, and so absurdly distinct in their duplicity that no amount of reasoning will correct the impression. It is too late for me now to try to teach these unequal eyes of mine to work together—the use of a concave glass, as above described, causing giddiness and pains in both eyes in a very short time. The complete disregard which either eye has of the work of the other is singular, and illustrates effectively some of the points noted above. When I am looking at distant objects, the left eye, though open, shows nothing (unless there happens to be a bright light in view on a dark ground, in which case a blurred image of the light is seen to the left of its position as seen distinctly with the right eye). On the other hand, when I am reading with the left eye (as I usually do, though not always) the right is perfectly at rest, though open. Once or twice, by the way, some rather amusing experiences have resulted from this remarkable difference, as when persons who have seen me reading with the page five inches from my eyes have taken it for granted I was exceedingly short-sighted, whereas with my right eye I have as good sight as most people for distant objects.

A MIRROR BAROMETER.

M. LEON TESSERENC DE BORT has ingeniously modified the common aneroid barometer by substituting for the train of clock-work terminating in a pointer a mirror mounted on a jeweled axis, which is rotated by the rise and fall of the exhausted receiver, and its indications read off by a small telescope by reflection from a graduated scale. The sensibility of the instrument is said to be much increased, and all errors due to a long train of wheelwork are eliminated.

ON THE DIFFERENT METHODS OF ARTIFICIAL ALIMENTATION.*

By THOMAS J. GALLAHER, M.D., of Pittsburg.

I PROPOSE to make a few remarks on the different methods of administering food aside from the voluntary method by the mouth. I propose also to say something of the substances to be employed in these various methods. This subject has not been treated with sufficient fullness in systematic treatises; but it has received considerable attention in medical periodical literature.

FEEDING BY INJECTIONS.

The most common method of artificial alimentation consists of injections into the rectum. Beef tea and broths of various kinds have been employed in this way from time immemorial, and instances innumerable are given where life has been sustained by these for long periods. Recent observations, however, do not fully confirm the great estimate put on these preparations as formerly employed.

It is now taught by many that the albuminous, caseous, and fatty elements of such enemas, without previous digestion, cannot be absorbed by the large intestines, and that, when these substances are introduced into the rectum unchanged, they are precipitated against the walls of the bowel, where they undergo fermentation and cause flatulency and pain. Dr. W. B. Richardson asserts that fatty substances, even after emulsification, cannot be absorbed because of the absence of lacteal vessels in the large intestine, by which alone they can be taken up.

Before finishing the subject of beef-tea injections, I will give some interesting experiments bearing on the subject of rectal digestion and absorption, from which imperfect conclusions may be drawn.

Experiments in reference to this subject were recently made in Germany by Czerny and Latchenberger. Five fistulous openings having formed in the lower part of the colon, in a gentleman who had long suffered from scrotal hernia which had inflamed and ulcerated, offered a good opportunity. The portion of the bowel experimented on extended from the lower of the fistulous openings in the groin to the sphincter muscles of the arms, a distance of twelve inches. It embraced therefore the rectum and a few inches of the sigmoid flexure of the colon. When water alone was injected, from 617 to 772 grains were absorbed in seven hours. The white of hard-boiled eggs cut into small fragments, shreds of fibrin, soluble albumen, starch, and fats did not dissolve or undergo any digestive changes.

The subjection of these substances to artificial digestion in mucus obtained from this part was attended by a similar result. Bits of the white of hard-boiled eggs inclosed in perforated capsules were introduced through the fistula into the bowel and retained there ten days without the slightest evidence of solution. It was observed, however, that all of these substances when previously subjected to artificial digestion, were absorbed.—(*American Journal of Medical Science*, April, 1874, from *Lancet*.)

ARTIFICIAL DIGESTION AND FOODS.

Dr. Williams relates a case of supposed gastric ulcer, where the weight of the patient was maintained by nutritive injections for a period of ten days. Beef tea, eggs, and brandy were the articles employed.—(*American Journal of Medical Science*, January, 1875, from *Lancet*.)

Some interesting experiments were made by Dr. Leube, of Erlangen, in 1873, chiefly upon dogs. He combined the pancreas of a pig or ox with various articles of food, and injected the mixture into the rectum of the dog. He found that digestion was performed and the nutritious elements absorbed.

The dog, by this method of feeding, retained its flesh and strength, while the evacuations were of proper consistence and smell. Applying the knowledge thus acquired to the human subject, he met with similar success by injecting into the rectum similarly prepared articles of food.

His experience enabled him to formulate the following preparation, which he preferred for anal injections: From $2\frac{1}{2}$ to 3 ounces of the pancreas of the pig or ox, carefully deprived of its fat and finely minced, were combined with from 5 to 9 ounces of beef, also finely minced and grated. These articles were well triturated together in a mortar with the addition of warm water, until the whole acquired the consistence of thick soup. The bowels having been evacuated an hour before, the whole amount was injected into the rectum at once with a syringe having a wide nozzle. The presence of this mass in the bowel, as a rule, produced no uneasiness, but would often remain from twelve to thirty-six hours without giving rise to a stool. Fat and starch were frequently added to the preparation before injecting.

Two cases, one affected with malignant disease at the upper part of the intestinal tube, and the other with gastric ulcer in which no food whatever could be retained upon the stomach, were subjected to this method of alimentation for a long time.

It proved, in his opinion, superior to all other means in maintaining the strength and flesh of the patients. The pulse became stronger and there was an improvement in the general condition and spirits of the patient.—("Half-Yearly Abstract," July, 1872, page 146.)

Recurring to beef tea as an article for rectal alimentation, the experiments of Czerny and Latchenberger show that neither fat, albumen, fibrin, starch, nor any saccharine substance which it may contain, can be digested to any extent by the rectal fluids; and that these substances as such cannot therefore gain entrance to the system by the rectum. It is true this bowel contains some follicles of Lieberkuhn and other glands by which a small amount of mucus and digestive fluid is normally secreted; but the amount is so small that its influence on digestion practically amounts to nothing. What then becomes of the beef tea or animal extracts when thrown unaltered into the bowels? The water, organic saline principles, and inorganic salts are certainly absorbed without change. These, of course, afford some nutrition to the body. But the albumen, fibrin, casein, and fat remain in the bowel for a while and then pass away. These substances must be digested artificially either outside of the body or while in the rectum by means of rennet, pepsine, pancreatine, etc., before they are in condition to enter the blood. After complete digestion the albuminoids are readily admitted to the circulation by capillary absorption, while the fatty matter can only be taken up to a very moderate extent. It seems certain that the existence of lacteals is not essential to a limited absorption of fatty emulsions.

In the preparation of this tea the meat should be cut very fine and allowed to macerate in cold or tepid water for not less than four or six hours. One pound of beef to one pint of water is the proportion generally employed. The whole may be boiled for thirty minutes or more. Some prefer using it uncooked. When sufficiently cooled, some of the digestive principles already alluded to should be added, and the whole gently agitated for an hour or so, when it is ready for use. Pepsine recently prepared from the stomach of the pig, calf, or sheep, according to the British formula, is preferable to the article sold in the shops.

As to Leube's preparation of grated raw meat, intimately combined by trituration with the pancreas of the pig or ox, I cannot speak in approving terms. I employed it on one occasion with uncertain results. Aside from the uncertain action of the pancreatic substance and juice on the albuminous ingredients of the meat, the difficulty experienced in making the preparation will always be an obstacle to its general adoption.

Defibrinated blood has been recently introduced as a valuable article for rectal alimentation. From 4 to 5 ounces are recommended to be injected at a time, to be repeated several times daily. Dr. A. H. Smith, in a report read before the New York Therapeutical Society, August 12, 1878 (*New York Medical Journal*), speaks of it in encouraging terms. Many of the elements of this injection will of course be admitted to the circulation, through the coats of the large intestine, but whether the serum and metalbumen, modifications of common albumen, can be absorbed without previous digestion, is a matter that can only be settled by further research.

Many cases come to the notice of the physician where, from stricture of the œsophagus, disease of the throat or mouth, or obstinate refusal on the part of the patient to take food, a sufficient amount of nourishment cannot be given to sustain life.

Where anal or renal disease exists, in conjunction with such cases, food cannot be introduced by the bowel. Other methods of alimentation must then be resorted to. A common expedient is to force the food into the stomach through the œsophagus tube. Dr. B. W. Richardson prefers the double to the single canula for this purpose, for by it food may be passed into the stomach by one chamber while the gastric gases are escaping by the other. In the use of the single tube these gases often interfere with the feeding.

FEEDING BY THE NOSE.

In consequence of the difficulty and sometimes danger of giving food in this way, feeding by the nose has been suggested and successfully carried into effect. For this purpose a syringe holding from 2 to 4 drachms may be employed. The patient, should he resist, must be placed in proper position, and held till the food is administered. One syringeful should then be gently injected into the nostril, then another, and another, until the amount desired be passed into the stomach. Time should be given between each syringeful for the patient to swallow and breathe.

All kinds of digestible food that can be reduced to a liquid or semi-solid state may be given in this way. In persons who offer no resistance, no difficulty whatever is experienced. The patient, however, must always lie upon his back, with head inclined backward and chin slightly elevated at every nasal feeding. Small children, even those at the breast, may easily be fed in this way.

Dr. Alexander Moxey, who was among the first, if not the very first, to call the attention of the profession (especially those engaged in the management of the insane) to the value of feeding by the nose, prefers passing the food through a funnel. After placing the patient in a proper position, he inserts the little end of a small Wedgwood-ware funnel just within the nostril, and gently pours the food into it, in small portions (two or four drachms) at a time. One pint or more may be introduced at one feeding, and the operation repeated two or three times a day, or as often as may be necessary. The mucous membrane of the nostril does not become irritated by the passing of food over it. Cold water causes more uneasiness than food. The articles should always be used while warm. For insane persons and those who are determined to commit suicide by starvation, this method is especially applicable. Many of these will take food in the natural way after having been subjected to this process a few times.

Dr. Tennant advocates the practice of feeding fever patients by the nose, and Dr. Hinchman favors nasal feeding among the obstinate insane.

This method has advantages over all others in many cases. The patient can always be easily restrained so that the nurse can operate with success, and the right kind of food can always be carried to the place where digestion and absorption are assured, with little or no risk to the patient. (*Lancet*, May, 1869, American edition.)

HYPODERMIC FEEDING.

Since the discovery of administering medicine by hypodermic injections, attempts have been made to administer food by the same process. Stricker and Aser some years ago injected peptones beneath the skin with the view of absorption and nutrition, and more recently Munzel and Perco, of Germany, succeeded in administering subcutaneously substances that had not undergone artificial digestion. Dogs were the chief animals operated on, but in one instance they injected nine grains of undigested food beneath the skin of a man. In these experiments fatty substances, such as olive oil, train oil, almond oil, etc., and subsequently milk, yolk of eggs, and dissolved sugar, were employed. The milk, eggs, and sugar were quickly absorbed, while the fatty matters disappeared more slowly. After 48 hours no trace of fat was discoverable. From 1 to 8 drachms of oil were injected at a time. (*American Journal of Medical Science*, October, 1869.)

Dr. James T. Whittaker reports a case of supposed gastric ulcer in a young man of twenty, in which nourishment was given exclusively by injections beneath the skin for a period of six days. Milk alternating with extract of beef was at first given, but in a day or two cod liver oil was substituted and used exclusively. From 2 to 3 ounces of oil were injected daily, and on one day as much as 4 ounces were given. No pain or uneasiness was occasioned by the presence of the oil, but two small abscesses arose from the milk injections. (*American Journal of Medical Science*, April, 1877.)

The most important and successful case of artificial feeding through the skin on record is reported by Dr. Kreng. The patient was an insane Hungarian, aged fifty-seven years. Having refused food, the doctor was obliged, for a period of 27 months, to support him by injections into the stomach through the usual œsophagal tube. At the end of this period a violent cough, an ejection of the food along the side of the tube, and threatening suffocation, attended every effort at giving food.

It was now determined to administer food subcutaneously. Accordingly from 15 to 20 cubic centimeters of olive oil were injected beneath the skin twice daily for a period of twenty days, no other nourishment having been given during that time. The oil certainly furnished him with nourishment, for he retained his flesh and strength. At the end of this period he ate without compulsion for some time, but on a short relapse occurring the injections were resumed. On one occasion the white and yolk of an egg beaten together were injected; but this was followed by an abscess. Each injection required from half an hour to an hour for its completion, and when the oil was injected slowly it gave rise to no pain. In this case it appears certain that the oil was absorbed and assimilated. (*New York Medical Journal*, March, 1876, from *Gazette Médicale de Paris*.)

The results of the experiments of Munzel and Perco, as well as the experience of Kreng and Whittaker, on the human subject, appear to be at variance with the experiments of Bernard and Barriswil; for while the latter found that crude albumen and gelatine were ejected from the blood without change when introduced into the veins, the former found their patients supported by these and other articles in the crude state when placed beneath the skin.

To reconcile this apparent discrepancy, it may be inferred that when crude articles in quantity are suddenly forced into the blood they are as suddenly forced out as foreign matter; but when these same articles are more slowly received into the blood vessels, as is the case in subcutaneous injections, the blood may have the power of digesting and fitting them for the assimilative process. There may be, therefore, such a thing as digestion in and by the blood.

FEEDING THROUGH THE SKIN.

It is believed by some that food may be introduced into the system by absorption through the skin. I have heard of children being bathed in beef tea under the impression that the tea was absorbed. It is probable that the water and organic salts are the only elements that enter the system.

This method of giving soups may, therefore, be condemned as comparatively useless. That oils can be admitted to the blood by friction I believe is a generally admitted fact. On many occasions I have applied cod-liver oil by friction to the skin of children suffering from marasmus, and have reason to believe with good effect. It is a matter of conjecture whether it enters the blood through the venous or lymphatic capillaries.

I have now imperfectly sketched the different methods of artificial alimentation, and have only further to say that two or more of these methods may be employed at the same time, if, in the opinion of the physician, the urgency of the case demands them.

Feeding by the nose and rectum, and through the œsophagus tube, must always be the chief artificial methods of administering food, since they are comparatively easy of performance and certain in results.

Besides the artificial methods of feeding above described, mention might be made of the transfusion of blood, and the intravenous injections of milk and peptones. As these subjects have recently occupied the attention of many eminent men, who are yet engaged in investigations, and as they do not strictly belong to the subject I have selected for the evening's paper, I will forbear giving the history and re-

* Read before the Bedford Medical Club. *N. Y. Medical Journal.*

sults of these operations. They are, however, of great importance, and deserve the attention of the profession. It is to be hoped that the investigations now being made will free them from the many errors and uncertainties by which they are surrounded, and place them on a sound and correct basis.

MORBID FEAR AS A SYMPTOM OF NERVOUS DISEASE.*

By GEORGE M. BEARD, M.D.

THE emotion of fear is normal to the human mind. It is as natural and as necessary to be afraid as to be courageous. Fear is, indeed, a part of the first law of nature, self-existence. The emotion is, therefore, physiological, varying both in degree and kind, with race, sex, age, and the individual. In neuropathology, especially in the pathology of functional nervous diseases, the difference between health and disease is of *degree* rather than of kind; the phenomena that belong to what we call health, passing by indefinite and not distinctly defined gradations into the phenomena of what we call disease; pathology being, in truth, as has been said, but the shady side of physiology.

Morbid fears are the result of various functional diseases of the nervous system, and imply a debility, a weakness, an incompetency and inadequacy, as compared with the normal state of the individual. A healthy man fears; but when he is functionally diseased in his nervous system he is liable to fear all the more; to have the normal, necessary fear of his physiological condition descend into an abnormal pathological state, simply from a lack of force in the disordered nervous system.

Thus it comes to pass that with the developments of functional nervous diseases in modern times, particularly with the increase of neurasthenia in its various phases, there has been an increase in the forms of morbid fears, and in the number of their manifestations. When any special phase of morbid fear assumes a considerable frequency and consistency, so as to allow of classification, it is proper and convenient to give it a special name by which it can be known, described, and referred to. With the understanding that these morbid fears are symptoms of diseases, rather than diseases of themselves, simply belonging to a large family of symptoms, it is a very important convenience to be able to recognize them, to interpret their meaning, to understand their relations to the other members of the same family of symptoms, and to be familiar with their diagnosis and treatment. It would probably be a correct statement to say that no symptom of functional nervous disease is so likely to be overlooked, or slighted, or misinterpreted, or improperly named, as this one symptom of morbid fear; it is diagnosticated as hysteria, hypochondria, dyspepsia, imagination, biliousness, and actual insanity. Insanity has, it is true, its morbid fears, but they are associated with delusions or hallucinations.

There are quite a number of varieties of morbid fear associated with cerebrasthenia, or brain exhaustion, without any hallucinations or delusions. The patient knows that there is no just, objective ground for his fear, but his emotional nature, under the influence of his exhausted nervous condition, overcomes his reason.

A number of years ago, I described a form of morbid fear under the term *astraphobia*, or *fear of lightning*, from the Greek *astrape* and *phobos*, fear. Of this disease I have seen quite a number of cases, and have nothing to say in regard to it beyond what has been already published.

The leading symptoms are headache, numbness and pain in the back of the head, nausea, vomiting, diarrhea, and, in some cases, convulsions. These symptoms are preceded and accompanied by great dread and fear. One of my patients tells me she is always watching the clouds in summer, fearing that a storm may come. She knows and says that this is absurd and ridiculous, but she declares she cannot help it. In this case the symptom was inherited from her grandmother; and even in her cradle, as she is informed by her mother, she suffered in the same way. A lady now under my care, the wife of a clergyman, was first attacked with these symptoms six years ago, in connection with other symptoms of general neurasthenic and uterine difficulties. Her husband tells me that on the approach of a thunder storm he is obliged to close the doors and windows, darken the room, and make things generally inconvenient for himself and family.

Westphal more recently has described a form of morbid fear under the term *agoraphobia*, or *fear of places*. This title, however, is quite inadequate to express the many varieties of morbid fear which the expression fear of places covers. The Greek word *agora*, from which Westphal derives his term, means an open square—a market place, a public place where assemblies were held—and as applied to the cases first described by him, the term is practically, though not etymologically, a correct one, for the fear of going across open squares or places, at a distance from houses or shops, was the chief feature in the cases described by him. This fear of open squares or places is, however, but one of a large number of phases that the fear of places assumes, as I have elsewhere described. In strictness, fear of places should be derived from the Greek word *topos*, place, a generic term, while *agora* is a special kind of place; *agoraphobia* would, therefore, be a species of *topophobia*, a general fear of places, which symptom seems to be capable of infinite variety. Thus one of my cases, a gentleman of middle life, could walk up Broadway without difficulty, because shops and stores, he said, offered him an opportunity of retreat, in case of peril. He could not, however, walk up Fifth avenue, where there are no stores, nor in side streets, unless they were very short. He could not pay a visit to the country in any direction, but was hopelessly shut up in the city during the hot weather. One time, in riding in the stage up Broadway, on turning into Madison square, he shrieked with terror, to the astonishment of the passengers. The man who possessed this interesting symptom, was tall, vigorous, full-faced, and physically and mentally capable of endurance. He had, however, other symptoms of cerebrasthenia.

These fears take opposite phases; thus, with one it is impossible to go to a certain place, where he was perhaps first attacked with the evil symptoms. And another finds it impossible or very difficult to go out of his house to any distance where business calls. I have now under care a patient who for a long time has been shut up in his house, unable to go anywhere, simply from fear of going anywhere. For a long time he was unable to come to consult me; but now I see him regularly; but he did not, until lately, since he has improved, go anywhere else. Quite a number of persons I have seen who find it difficult to go on long journeys, and if they do go, must have company. A person wrote me from a distant city in the West, expressing a desire to come and consult me, but upon reaching a city at some distance was compelled to return home without reaching New York. All these forms of morbid fear—fear of leaving home, fear of going to any locality or in any direction, fear of travel—are properly varieties of *topophobia*, the fear of open squares or places being relegated, though not quite correctly, by *agoraphobia*.*

Dr. Meschede brought to the attention of the physicians at Cassels, in Germany, a form of morbid fear quite the opposite of what is known as agoraphobia, or fear of *open* places. In his case the symptom was fear of *close*, *narrow* places. The patient, a young man twenty years of age, was seized with a feeling of giddiness and confusion when in a small, narrow room. In the summer he could not sleep in a room at all, but was obliged to camp out; in winter he slept in a large, airy room. He was obliged to give up his studies and become a farmer. This symptom cannot be classed as agoraphobia at all, for it is the reverse condition. It belongs properly to what I call *topophobia*, fear of places; and is, like agoraphobia, a species of which topophobia is the genus.

A form of fear I have lately described is *anthropophobia*, derived from the Greek *anthropos*, man, and *phobos*, fear. This term applies to aversion to society, a fear of seeing, encountering, or mingling with a multitude, or of meeting any one besides ourselves. This phase of morbid fear has different varieties. One form is *gynephobia*, *fear of women*, from the Greek *gune*, woman, and *phobos*, fear.

Some patients afflicted with cerebrasthenia have no fear of male society, but are particularly timid at even the approach of females. They can mingle with men in ordinary business relations, but dread to go in any company where women are found, even when not particularly bashful. A person once consulted me for gynephobia which took on a peculiar form, he being only afraid of women in the society in which he moved; women of the lower order he cared nothing for, and he had no anthropophobia, or fear of man. In quite a number of cases this fear of man is so severe as to compel patients to give up business entirely; and I know a number of cases where men of strong muscles and having the appearance of great physical strength have been compelled through this symptom alone to withdraw from the occupations in which they were engaged; they could not face men, deal with them, persuade them to buy or sell, or have any influence over them; they dreaded to meet a human being. This form of morbid fear is often accompanied with turning away of the eyes and hanging down of the head, but not necessarily so, and usually so only in the severer cases. This phase of morbid fear is a very good barometer of the condition of the system. From this alone we can often judge whether the patient is improving or growing worse. It is a very interesting symptom. In some cases I hold the head of the patient between my hands, so as to bring his face opposite mine, and even then he will involuntarily turn away his eyes. This phase of morbid fear also has its opposite. In some persons there exists what may be called *monophobia*, or fear of being alone. Some of these persons cannot travel alone, but have no difficulty in traveling if they are in company with some one. Sometimes they cannot walk the street alone or leave the house except in company.

A form of morbid fear that has long been known to the profession is pathophobia, or fear of diseases—more commonly known as hypochondriasis. This form of morbid fear seldom exists alone, but is found in company with other symptoms—some real disorder of the nervous system. The pathophobic sufferer, with brain or stomach, or both, exhausted for some reason, may fear disease of the heart, of the stomach, or of the brain, or of the reproductive system, even when there is no sign of disease except his fear. The mistake usually made in the study of these cases is to assume that this fear of disease is the only symptom which the patient has, and that it is the cause of the disease; whereas, usually, it is the result of the disease, whatever the cause may be; and as such should be studied and treated.

There is a manifestation of morbid fear which is not uncommon, and to which we might perhaps give the term *pantophobia*, or fear of everything; all responsibility, every attempt to make a change of movement being the result of dread and alarm. The wife of one of my patients has a morbid fear in reference to one of her sons, a lad of about fifteen years of age; and so distressed is she by it that she cannot allow him to go out of the house or out of her sight, fearing lest he may be kidnaped, or some harm may come to him, as in the case of Charley Ross. The poor fellow is thus kept a prisoner most of the time, and the whole family is disturbed and annoyed. He must remain in the city during the summer, as she cannot allow him to leave town; and at no season can he go anywhere unless accompanied by his tutor.

A lady now under my treatment who is also *astraphobic*, tells me that she is afraid to go into the street, to do any shopping, or attend to any business; that it is an affliction for her to come to see a physician; everything is a dread to her, even when there is no draught made upon her physical strength.

The expression *phobophobia*—fear of fears—might possibly apply to a certain class of nervous patients, who fear they may fear, provided they make an attempt to move or go in any direction where their mobid fear is in the way; they are afraid even when they do and say nothing. These persons fear when they are entirely still and inactive, from a fear that if they attempt to do anything they will be attacked with their especial morbid fear. One of my patients—a stout, and large man—in addition to topophobia (fear of places) had at one time a fear of committing some crime against women that would disgrace him. He was ashamed of his fear; he could not help it, although he has now entirely recovered.

Mysophobia, fear of contamination, lately described by Dr. Hammond, comes under this head; the results of the treatment showing very clearly that it is symptomatic of a similar or analogous condition of the brain. In those cases there were no hallucinations or delusions.

In regard to all these different forms of morbid fear, by whatever name they are known or described, these general propositions are true and verifiable.

First.—These morbid fears are symptomatic of functional, never or rarely of organic diseases. The existence of any of these symptoms in a doubtful case of diagnosis, would alone almost establish the nature of the disease, or enable us to give the casting vote.

The best test of skill in the practice of neurology is in making a differential diagnosis between functional and organic diseases in their early stages; for this cause alone morbid fears demand close attentions.

While it is possible for hysterical and neurasthenic symptoms to appear and maintain themselves, more or less, in organic diseases, yet these symptoms of morbid fear are not found, according to my observation, in what we call organic or structural diseases of the brain or spinal cord; it is strange that they are not, but the fact as here related is verifiable.

They are not found in insanity itself, and the habit of calling them forms of mania or delusion is not based on fact or a right study of these cases. I observe that even now some forms of morbid fears are classed under insanity, or mania of some kind, even when there are no delusions or hallucinations. When the insane have morbid fears such as I have described, or very many others which they may have, and do have, as we all know, they are delusions out of which they cannot be reasoned, and are a part of, and in harmony with other delusions of the insane. But in all the cases to which I have here referred, there are no hallucinations whatever; the patient is as well aware of his delusion as his friends are, and is as anxious to get rid of them as he would be of a sick headache, fever, or paralysis; but he is unable to shake them off until his exhausted brain, of which they are the direct result, is strengthened by hygiene and time treatment.

Second.—These symptoms may come on suddenly, in some cases almost instantaneously, and when once they appear, they may exist for months and years, varying in intensity at different times, like other symptoms of cerebrasthenia, with which they are often associated.

Third.—These morbid fears are very frequently, though not always or necessarily, the result in whole or part of disorder of the reproductive system.

Excess in the male in the natural or unnatural ways, or prolonged and teasing continence united with sexual excitation, and, in the female, various slight and superficial uterine erosions, or displacements or lacerations, are the common provoking uses of these morbid fears, especially in constitutions where the nervous diathesis predominates.

These fears may exist long after the local difficulty has been cured; in this respect these symptoms follow the law of the nervous symptoms with which they are so often associated. Some of these cases are anæmic, but the majority are not so, and many are models of physical strength.

Fourth.—These morbid fears rarely exist alone. They almost always appear in connection with other symptoms of neurasthenia, either myelasthenia, exhaustion of the spine, or cerebrasthenia, exhaustion of the brain, most frequently the latter. I think, indeed, that I have never seen a case of morbid fear, such as I have here described, that existed alone, without some one accompanying neurasthenic symptom, or many such symptoms. In some cases, I admit, these accompanying symptoms are few and slight, and can be ascertained only by careful study.

Among those associated symptoms may be mentioned palmar-hyperidrosis, flushing of the face, a feeling of profound exhaustion, insomnia, hopelessness, shooting pains in the extremities, excess of oxalates and urates in the urine, heaviness of the loins and limbs, dilated pupils, local spasms of muscles. Only rarely, however, is there a complete picture in which all these symptoms are represented. Like all these symptoms of neurasthenia morbid fears very often occur in those of great, even enormous muscular strength and endurance; many of them can walk and work all day with muscle and with brain, but in the presence of their special fears they are as infants.

A very frequent accompanying symptom is dizziness. Many of these cases, when they approach the object of dread, or even think of approaching it, are seized with vertigo—sometimes with less defined abnormal sensations. I have seen three cases where an epigastric spasm appears on attempting or even thinking of doing anything which is a dread. I have now under care a patient who tells me that he has a spasm in the stomach whenever he thinks of doing anything where he fears a failure. He describes it as a sudden sinking, a falling, somewhere between the base of the lungs and the navel.

This patient has also a large array of correlated nervous symptoms, such as sweating of the hands, twitching of the eyelids, mental depression, etc. One of these cases had this symptom of spasm—sinking in of the stomach—while at school, and it would come upon him whenever he was called upon, or feared he might be called upon, to recite; even the thought of responsibility, though it might be in the remote future, brought on the attack. The very existence of a morbid fear suggests to us that we search for other symptoms.

Fifth.—The treatment of morbid fear is the treatment of the condition of the brain, of which it is a symptom of the local or general condition on which the brain exhaustion depends; very generally stated, this condition requires both constitutional and local treatment. The constitutional treatment includes the whole array of sedatives and tonics, the more effective being the bromides and electricity; and counter-irritation at the back of the neck and in the bowels by means of cathartics. The local treatment in cases of disorder of the prostatic urethra in males consists in my own practice of the following procedures: very mild electrolysis with the urethral electrode—application of liquor bismuthi—of iodoform, by suppositories, by sounds, and *dry* cold in the urethra and the rectum.

These cases can be cured, and be permanently cured, but cannot be cured suddenly nor usually by a single prescription. They have been sick before we see them for months and oftentimes for years. The details of the treatment must be varied with the idiosyncrasy of the patient. The causes of failure are three-fold. First, the exclusive use of general treatment by medication, the local irritation from which the symptoms start being undetected. Secondly, the use of stimulants where sedative treatment is required. Thirdly, the want of change in the modes and details of treatment, and perseverance in their use.—*The Hospital Gazette.*

* Read at the annual meeting of the American Neurological Association, June 18th, 1879.

* In etymological strictness agoraphobia means fear of *large assemblies* of human beings, and not of the place where the people meet.

MAMMARY INFLAMMATION TREATED BY ICE.

A WRITER in the *British Medical Journal* says that the suggestion of treating threatened inflammation of the mammary gland by ice was "one of the most valuable hints he ever got." He thus describes its use in a case:

"I used a large Chapman's spine-bag, filled with ice, which encircled the lower half of the breast. It felt very cold indeed for a minute or two, then a considerable quantity of milk was shot out, as from a syringe (no milk had flowed before), the pain abated, and in an hour was almost gone; I now renewed the ice in the bag, and the patient kept it closely applied with her arm, which was protected from the cold by a folded towel. Next morning, I found her hugging the ice-bag and loud in its praise. She continued suckling her infant; but she suggested that the baby should not be put to the breast oftener than two or three times in the twenty-four hours. On the fourth day after the commence-

ment of the ice the most careful examination failed to detect anything wrong in the breast, and she is now quite well and nursing her child. No other remedies were used."

THE SECRETION OF THE GASTRIC GLANDS.

PROFESSOR HEIDENHAIN succeeded in separating a considerable portion of the fundus of the stomach in a dog from its connection with the rest of the organ, and forming it into a blind sac communicating with the exterior of the body. This enabled him to collect the secretion of the gastric glands unmixed with that of the pyloric glands, and uncontaminated by the saliva and other liquids which pass down the œsophagus. The secretion is a clear, strongly acid liquid, containing an unexpectedly small amount of mucus, and an average of 0·45 per cent. of solid matter, partly organic, partly inorganic, the former consisting mainly of pepsine. The average acid'ty of the liquid is equivalent to 0 52 per cent. of hydrochloric acid, which is far higher than that of the mixed gastric juice, free from saliva, examined by Bidder and Schmidt. Richet, from observations on the juice of a man with a gastric fistula, found that when fresh it contained only hydrochloric acid, while when kept for a time it developed an organic acid, probably sarcolactic. No such acid was observed to be produced in the secretion obtained from the dog. It was found that the introduction of nutritious food into the stomach induced active secretion in from fifteen to thirty minutes, and this continued until the stomach had completely emptied itself. But if indigestible substances were introduced no secretion flowed from the sac for upwards of an hour. Water was then given to the animal, and secretion commenced, but only lasted an hour and a half.

From these and other experiments, Professor Heidenhain concludes that mechanical stimulation of the stomach excites secretion only at the point of contact, general activity of the grandular apparatus requiring absorption for its production. If the composition of the secret liquid be examined at regular intervals during the digestive process, its acidity is found to remain pretty uniform, but the proportion of pepsine contained in it undergoes a peculiar and orderly series of variations. During the second hour it sinks rapidly to a minimum; towards the fourth or fifth hour it rises again to a point generally higher than at first, and remains at or near this point for a considerable time. These variations are quite independent of the amount of pepsine actually contained in the glands, which is known to sink steadily. The secreting surface can pour out a liquid very rich in pepsine at a time when its poverty in this substance is most strongly marked. No definite conclusion can at present be arrived at as to the cause of this phenomenon.—*Pflüger's Archivi Academy.*

MELTING POINTS OF THE ELEMENTS AND THEIR COEFFICIENTS OF EXPANSION BY HEAT.

By T. CARNELLEY.

A TABLE which the author has compiled shows that with five exceptions the coefficient of expansion increases as the melting point sinks. The five exceptions are arsenic, antimony, bismuth, tellurium, and tin; the three former of which belong to the same elementary group, and even these among themselves display a similar relation between their melting points and coefficients of expansion. Why these five bodies form an exception does not yet appear. It must, however, be noted that they are all found on the ascending side of Meyer's Curve of the Elements (see his "Modern Theories of Chemistry"), whilst three of them, tin, antimony, and tellurium, follow immediately upon each other in the above curve. Arsenic, antimony, bismuth, and tellurium are all very brittle and belong to the same crystalline system; and bismuth and antimony are the only two known pure elementary bodies which expand on congealing. Tin, in some of its compounds, displays an abnormal melting point, as will be shown in a future memoir. Both the melting points and the coefficients of expansion may be periodic functions of the atomic weight.

PURIFICATION OF PLATINUM AND IRIDIUM.

AN alloy of these metals is used in the manufacture of the standard weights and measures. In a paper recently communicated to the Royal Society, Mr. G. Matthey describes the methods he employed for preparing the metals in a state of purity.

The following is an abstract:

The six metals (of which platinum is the chief) usually found more or less in association, present characteristics of interest beyond their metallurgical utility, which are, perhaps, worth alluding to. It is, for instance, a curious fact that the group should consist of three light and three heavy metals, each division being of approximately the same specific gravity—the heavier having (in round figures) just double the density of the lighter series.

Thus we find osmium, iridium, platinum, forming the first division, of the respective specific gravities of 22·43, 22·39, 21·46; whilst ruthenium, rhodium, and palladium are represented by the figures 11·40, 11·36, 11, the average densities of the heavy and light divisions thus being respectively 22·43 and 11·25.

But a more interesting and important classification is what I may designate as a first and second class series, from the more important view of their relative properties of stability. Thus platinum, palladium, and rhodium form the first or higher class, not being volatilizable in a state of oxide; iridium, osmium, and ruthenium forming the second or lower class, their oxides being more or less readily volatilized.

The oxide of iridium is affected at 700° to 800° C., and entirely decomposed at 1,000°, whilst osmic and hyporuthenic acids are volatilized at the low degree of 100°, the latter exploding at 108°. The chlorides of these metals can be sublimed at different temperatures (as also the protochloride of platinum).

PLATINUM.

The preparation of this metal in a state of purity is an operation of extreme delicacy. I commence by taking ordinary commercial platinum; I melt this with six times its weight of lead of ascertained purity, and after granulation, dissolve slowly in nitric acid diluted in the proportion of 1 volume to 8 of distilled water. The more readily to insure dissolution, it is well to place the granulated alloy in porcelain baskets such as are used in the manufacture of chlorine gas for holding the oxide of manganese. When the first charge of acid is sufficiently saturated, a fresh quantity should be added until no more action is apparent; at this stage the greater part of the lead will have been dissolved out, together with a portion of any copper, iron, palladium, or rhodium that may have been present. These metals are subsequently extracted from the mother-liquors, the nitrate of lead by crystallization, and the remaining metals by well known methods.

The metallic residue now obtained will be found in the state of an amorphous black powder (a form most suitable for further treatment), consisting of platinum, lead, and small proportions of the other metals originally present—the iridium existing as a brilliant crystalline substance insoluble in nitric acid. After digesting this compound in weak aqua regia, an immediate dissolution takes place of the platinum and lead, leaving the iridium still impure, but effecting a complete separation of the platinum.

To the chloride of platinum and lead after evaporation is added sufficient sulphuric acid to effect the precipitation of the whole of the lead as a sulphate, and the chloride of platinum, after dissolution in distilled water, is treated with an excess of chloride of ammonium and sodium, the excess being necessary in order that the precipitated yellow double salt may remain in a saturated solution of the precipitant; the whole is then heated to about 80°, and allowed to stand for some days; the ammonio-chloride of platinum will settle down as a firm deposit at the bottom of the vessel, whilst if any rhodium, as is generally the case, is present, the surface liquor will be colored a rose tint, occasioned by a combination of the salts of the two metals.

The precipitate must be repeatedly washed with a saturated solution of chloride of ammonium and subsequently with distilled water charged with pure hydrochloric acid. This is necessary for its purification. The small quantity of the double salt which will be taken up and held in solution is of course recovered afterwards. Rhodium may still exist in the washed precipitate, which must therefore not be reduced to the metallic state until its separation is completed, and this is the best effected by mixing with the dried compound salts of chloro-platinate and chloro-rhodiate of ammonia, bi-sulphate of potash with a small proportion of bi-sulphate of ammonia, and subjecting to a gradual heat brought by degrees up to a dull red in a platinum capsule, over which is placed an inverted glass funnel. The platinum is thus slowly reduced to a black spongy porous condition freed from water, nitrogen, sulphate of ammonia, and hydrochloric acid, the rhodium remaining in a soluble state as bi-sulphate of rhodium and potash, which can be dissolved out completely by digesting in boiling distilled water; a small quantity of platinum will have been taken up in the state of sulphate, but is regained by heating the residue (obtained on evaporation) to redness, which reduces it to the metallic condition, the rhodium salt remaining decomposed.

By the method above described the platinum is freed not only from rhodium, but from all other metals with which it may have been contaminated, and is brought to a state of absolute purity, of the density 21·46, the highest degree obtainable.

IRIDIUM.

In practice, the purest iridium which can be obtained from its ordinary solution (deprived of osmium by long boiling in aqua regia and precipitated by chloride of ammonium) will almost invariably contain traces of platinum, rhodium, ruthenium, and iron.

I fuse such iridium in a fine state of division with ten times its weight of lead, keeping it in a molten state for some hours, dissolve out the lead with nitric acid, subject the residue to a prolonged digestion in aqua regia, and obtain a crystalline mass composed of iridium, rhodium, ruthenium, and iron, in a condition suitable for my further treatment. By fusion at a high temperature with an admixture of bi-sulphate of potash, the rhodium is almost entirely removed, any remaining trace being taken up together with the iron in a later operation. The iridium so far prepared is melted with ten times its weight of dry caustic potash, and three times its weight of niter, in a gold pan or crucible; the process being prolonged for a considerable time to effect the complete transformation of the material into iridiate and ruthenate of potash, and the oxidation of the iron; when cold, the mixture is treated with cold distilled water. The iridiate of potash of a blue tinge will remain as a deposit almost insoluble in water, more especially if slightly alkaline, and also the oxide of iron.

This precipitate must be well washed with water charged with a little potash and hypochlorite of soda until the washings are no longer colored, and then several times with distilled water.

The blue powder is then mixed with water strongly charged with hypochlorite of soda, and allowed to remain for a time cold, then warmed in a distilling vessel, and finally brought up to boiling point until the distillate no longer colors red, weak alcohol acidulated with hydrochloric acid.

The residue is again heated with niter and potash water charged with hypochlorite of soda and chlorine, until the last trace of ruthenium has disappeared.

Further, to carry out the purification, the blue powder (oxide of iridium) is redissolved in aqua regia, evaporated to dryness, redissolved in water, and filtered.

The dark colored solution thus obtained is slowly poured into a concentrated solution of soda and mixed with hypochlorite of soda, and should remain as clear solution without any perceptible precipitate, and subjected in a distilling apparatus to a stream of chlorine gas, should not show a trace of ruthenium when hydrochloric acid and alcohol are introduced into the receiver. In this operation the chlorine precipitates the greater part of the iridium in a state of blue oxide, which, after being collected, washed, and dried, is placed in a porcelain or glass tube, and subjected to the combined action of oxide of carbon and carbonic acid obtained by means of a mixture of oxalic with sulphuric acid gently heated.

The oxide of iridium is reduced by the action of the gas leaving the oxide of iron intact; the mass is then heated to redness with bi-sulphate of potash (which will take up the iron and any remaining trace of rhodium), and after subjecting it to many washings with distilled water, the residue is washed with chlorine water to remove any trace of gold, and finally with hydrofluoric acid, in order to take out any silica which might have been accidentally introduced with the alkalies employed or have come off the vessels used.

The iridium, after calcination at a strong heat in a charcoal crucible, is melted into an ingot.

ALLOY OF IRIDIO-PLATINUM.

Operating upon a charge of 450 ounces of platinum and 55 ounces of iridium, I commenced by melting these metals together and casting into an ingot of suitable shape, which I then cut into small pieces with hydraulic machinery. After remelting and retaining in a molten condition under a powerful flame of oxygen and common gas for a considerable time, I recast and forged the mass at an intense white heat under a steam hammer, the highly polished surfaces of which were cleaned and polished after each series of blows—when sufficiently reduced the alloy was passed through bright polished steel rollers, cut into narrow strips, and again slowly melted in a properly shaped mould, in which it was allowed to cool. I thus obtained a mass of suitable shape for forging, perfectly solid, homogeneous, free from fissures or air holes, and with a bright and clean surface.

A piece cut from the end of a mass so prepared was presented to the French Academy of Science, and gave the following results:

Weight in air	116·898	grms.
" water	111·469	"
Showing a density of	21·516	"

thus proving that the necessary processes of annealing at a high temperature had caused it to resume its original density.

The analysis gave—

	1	2
Platinum	89·40	89·42
Iridium	10·16	10·22
Rhodium	0·18	0·16
Ruthenium	0 10	0.10
Iron	0·06	0 06
	99·90	99·96

From which is deduced—

	Proportion.	Density at zero.	Volume.
Iridio-platinum, at 10 per cent.	99·33	21 575	4·603
Iridium, in excess	0·23	22·380	0·010
Rhodium	0·18	12·000	0·015
Ruthenium	0 10	12·261	0·008
Iron	0·06	7·700	0·008
	99·90		4·644

Density at zero, calculated after No. 1 analysis, 21·510
Density at zero, calculated after No. 2 " 21·515

which coincide perfectly with the practical results obtained."

MM. Deville and Mascart find the coefficient of dilatation to be from 0° to 16° C. 0·00002541.

ON ULTRAMARINE.

By M. T. MOREL.

THE qualities which are commercially required in ultramarines are a deep and brilliant shade, fineness, coloring power, resistance to acids, and resistance to alum. All the trials made on this subject are simply comparative, and refer to a sample which has already been chosen as a standard.

Shade.—The two samples are placed side by side upon white paper, by means of a spatula (at a *north* window). If one of the samples is darker than the other, this is best ascertained by crushing a small quantity of this upon the other.

Fineness can scarcely be recognized except by feel. Only two other methods are known which may be used, and both are apt to lead to inaccurate results.

It has been proposed to stir up the ultramarine in water, and note the time which it takes to settle. But this time varies for one and the same ultramarine, according to the manner of manipulation, and often a very fine ultramarine settles more rapidly than a coarse one of another make.

Ultramarines may also be diluted with white powders, comparing the intensity of the blues thus let down. This method does not indicate absolutely the fineness, but is, properly speaking, a measure of the coloring power. We may draw from it useful indications; thus, in general, the finer an ultramarine the more it retains its blue shade when diluted with white. This observation is applicable only when the ultramarines do not differ too much in tone.

Coloring power is best recognized by mixing with a certain quantity of a white powder, the finer the better, such as kaolin. One part of the blue is mixed with six parts of the white, and the ultramarine thus let down is compared with the standard sample, which has been treated in the same manner. To have results exactly comparable, it is necessary to do all the weighings at once, as both ultramarine and kaolin absorb moisture from the air. The tints obtained may be either of a pure blue, or of a greenish, a violet, or a rose blue, according to the nature of the sample. Thus the comparison of the different kinds is rendered very difficult. (What is the difference between a violet blue and a reddish or rose blue?)

Resistance to acids is a necessary quality for ultramarines, which have in their use to come in contact with liquids which are either acid or capable of becoming so. This occurs in the pigment style of printing, where the albumen and other materials which serve to fix ultramarine upon the fiber are rapidly decomposed and turn acid. To come as close as possible to the actual condition of the case, the trial is made with a solution of oxalic acid, which is one of the products of the decomposition of albumen. A standard solution is made by dissolving 1¾ oz. of crystalline oxalic acid in 35 fluid ounces of water.

The one-hundredth part by measure of this solution is put into a test tube along with 7·7 grains of the ultramarine, and is well shaken up, while the change which ensues is compared with what takes place in a portion of the standard ultramarine similarly treated.

Resistance to Alum.—Formerly for ultramarine used in the paper manufacture resistance to alum was the first consideration. Now it is found that, if the paper is manufactured under right conditions, the most delicate ultramarines are not affected.

Still, if it is desired to measure this resistance, we make a saturated solution of alum, and pour 1½-hundredth parts of 35 fluid oz. thereof into a test tube, along with 7·7 grains of the sample under examination, making, of course, a comparative experiment with the standard sample. It is necessary to shake up frequently, lest the ultramarine should form a clot.

It is generally believed that an ultramarine which resists alum will resist acids equally well. This is an error: the resistance to alum is quite different from the resistance to acids.

In the paper trade coloring power is the first condition required. The greatest possible fineness is needful to avoid blurs. If much alum is used the resistance of the sample to alum should be tried.

In printing fineness is a main point, especially for dark-

colored ultramarines. Resistance to acids should also be tested.

For bluing bleached goods the ultramarine may be tried by steeping a lot of the yarns to be tinted in a bath made up of 15 grains of the sample and 7 fluid ounces of water. The yarns are then dried and repeatedly passed between the fingers to try if the color adheres to the fiber, which is a mark of fineness.

Examination of Lump Blues (ball blues, thumb blues, fig blues, etc.).—Fifteen grains are placed as above in seven ounces of water, and left to steep for a day, taking care to crush the fragments. A small parcel of cotton is then steeped in the liquid.

By the fracture of a ball it is often possible to judge of its quality. If the blue is mixed with a coarse white this is perceived by simple inspection of the broken part (especially with the aid of a lens). If the white is fine the fracture must be scraped with the nail or a penknife, and on the flat surface thus formed very small traces of white matter may be detected.

The following directions for the examination of the blues of commerce may be useful to bleachers, paper makers, etc.

Mix up the powder with concentrated sulphuric acid.

If the color is destroyed, it is *Prussian blue.*

If, on the contrary, the blue color remains unchanged, dilute the mixture with water. If the color is then dissolved with a smell of sulphureted hydrogen, it is *ultramarine.*

If there is no decoloration on adding water, and the blue either is or becomes soluble, it is a preparation of *indigo.*

If, in spite of sulphuric acid and water, the blue remains insoluble, it is *cobalt.*

If the original color, on mixture with sulphuric acid, turns a reddish brown, water is added. If the coloring matter is thereby precipitated in wine-red flakes, leaving a colorless liquid, it is *anthracene blue* or *alizarine blue.*

If, on the addition of water, the original blue color returns, it is *aniline blue.*

If, on treatment with sulphuric acid, a green liquid is produced, we have *methylene blue.*

We must add that the crude indigo of commerce, when mixed with sulphuric acid, though it remains a dark color in the mass, takes a yellow color at the edges, then turns greenish, and does not become fairly blue till after some time. If the solution is diluted with water before this change has taken place, much of the indigo is reprecipitated.

Anthracene blue is sold in the form of a brown powder, with coppery reflections, suspended in water. This paste may be at once treated with sulphuric acid, and dissolves then very readily.

The following reactions serve to confirm the tests given above.

Among all these blues alcohol dissolves only two—aniline blue and methylene blue.

Dry extract of indigo, though insoluble in alcohol at 90 per cent., gives off some traces of coloring matter, and even absolute alcohol is also slightly colored, the water contained in the alcohol dissolving doubtless the extract. (Absolute alcohol seldom retains long its perfect freedom from water.)

A solution of oxalic acid behaves like water with all these blues, except ultramarine, which it destroys, and Prussian blue, which it dissolves.

A caustic soda lye turns anthracene blue in paste or suspended in water to a green; it destroys Prussian blue, and turns extract of indigo yellow. The original color reappears on neutralizing the soda with an acid.

Fuming nitric acid, added in small quantity to water holding these blues in suspension or solution, destroys completely only indigo, anthracene blue, and ultramarine.

Cobalt blue (smalt) resists all reagents.

The following instructions will serve to distinguish these blues when present as dyes or tints upon textile fabrics or paper:

The swatch is first steeped in dilute muriatic acid.

If the shade turns to a red or orange we have *logwood* or *orchil.*

If the color is destroyed with a smell of sulphureted hydrogen—*ultramarine.*

If the muriatic acid takes no effect, steep in soda lye. If this changes the color to red—*aniline blue.*

If to yellow—*extract of indigo.*

If to violet—*methylene blue.*

If the color is destroyed—*Prussian blue.*

If the soda lye does not modify the color, treat the swatch with nitric acid diluted with an equal bulk of water.

If the color disappears—*indigo.*

If unchanged—*cobalt.*

Paper or tissues tinted with aniline blue are perceptibly affected on exposure to the sun for a few hours. No other color is attacked so rapidly.

Papers or tissues blued with ultramarine or cobalt leave a blue ash if burnt.

If ultramarine has been used, these ashes are decolorized by dilute acids, which merely brighten cobalt.

If Prussian blue has been employed, the ash gives the usual reactions of iron.—*Moniteur Scientifique.—Chemical Review.*

ULTRAMARINE.

By E. W. BUCHNER.

BY heating sodium, aluminum, and silicum in a current of sulphureted hydrogen, the author obtained a black mass, which, on extraction with water and reheating, with free exposure to the air, became converted into ultramarine blue.

SEPARATION OF FERRIC OXIDE AND ALUMINA FROM MANGANESE.

A. CLASSEN'S method is based upon the fact that solutions of manganic salts are precipitated by neutral potassium oxalate, the precipitate being soluble in an excess of the latter. The potassium-manganic oxalate is then decomposed by concentrated acetic acid, in which manganese oxalate is insoluble, while potassium ferric oxalate formed in the same manner gives no precipitation with acetic acid. To obviate the disturbing action of the alkaline chlorides which prevent the complete precipitation of the manganese oxalate, Classen adds zinc chloride in excess, so that the ZnO may be three or four times the quantity of the MnO. All the manganese is thus precipitated with the zinc oxalate, the cold bulky precipitate is rendered dense and crystalline by heating to 40° to 50°, and is then washed, dried, and ignited with excess of air, forming a mixture of manganese and zinc oxides, which often, in consequence of imperfect washing, contain traces of potassium permanganate. The contents of the crucible, after a short ignition, are digested with alcohol and hot water, and washed again.

The ignited mixture of Mn_3O_4 and ZnO is decomposed by concentrated hydrochloric acid; the chlorine set free is passed into solution of potassium iodide, and the iodine which separates is titrated with sodium hyposulphite. The filtrate from the oxalates contains ferric oxide and alumina, which, after concentration, are precipitated with alcohol and a little acetic acid, and the ignited precipitate is washed with water containing a little potassium carbonate. The method is likewise suitable for separating calcium, cobalt, nickel, and probably copper from ferric oxide and alumina.—*Zeit. Anal. Chemie.*

TURKEY RED DYEING.

DR. A. MULLER-JACOBS, in an article "On the Use of Tannin in Turkey Red Dyeing on the New Principle," says: "The whole art of dyeing, and especially Turkey red dyeing, depends essentially on a dialyzing process, and the object of the oiling or mordanting is merely to convert the vegetable fiber into the best possible dialyzer. Parchment (paper?) and mercerized cotton are dialyzers, and both possess in a high degree the property of taking up without mordant such colors as dye without base. The white mordanted tissue (oiled on the old principle) serves as a dialyzer for solutions of alumina.

"Simultaneously with the dissociation of the aluminum salts, the hydrate of alumina combines with the fixed oleic acid. Of course, in this process the chemical affinity of the fixed oil for aluminic salts cannot be overlooked. Mercerized cotton likewise decomposes salts of alumina, and this is still more completely effected by cellulose, first mercerized and then oiled on the new method. The process in the dye bath depends still more upon dialysis than does the mordanting process. The most fiery red is obtained by converting the goods into the best possible dialyzer. The *chemical* union of the alizarin with the mordants occurs only at the more elevated temperatures of the dye-beck, when the whole of the coloring matter has been absorbed, and the bath has become almost colorless.

"The more readily the cotton is dialyzed by the mordanted cotton (which occurs at low temperatures), *i. e.*, the better the coloring matter 'works on' at a *low* temperature, the more excellent is the red. Additions of chalk, of aluminous salts, etc., even in the smallest quantity, alter essentially the liquid to be dialyzed, and have thus a powerful influence on the color to be produced. This seems to show that the process of dyeing is physical rather than chemical in its nature. On the addition of small quantities of chalk to the dye bath, *e. g.*, ¼ lb. to 100 lb. goods, the coloring matter works on much more quickly, the dialysis being accelerated; while, on the other hand, the addition of Turkey red oil, of acids, or alkalies, even in the smallest quantities, or of certain salts, prolongs or even prevents the dialysis. Cotton, if possible, slightly mercerized and treated with Turkey red oil, receives by the aid of tannin the power to dissociate aluminic compounds as in the old process, and to fix the hydrate upon the fiber in the state best suited for the production of brilliant colors.

"By the addition of tannin we compensate for various defects relating to dialysis, and render the addition of Turkey red oil to the dye bath possible, whereby the alizarin is absorbed more slowly, and a greater equality in the color is obtained. By fixing large quantities of alumina in the process of mordanting we produce a very fiery red, which lies more on the surface, while the fiber remains white internally, and the color scarcely bears a strong soaping. This evil may be avoided by adding tannin to the dye bath along with Turkey red oil, thus retarding the dialysis, and giving the color time and opportunity to penetrate into the interior of the fiber. Without tannin we should produce a tawny fugitive red; the addition of chalk (without oil and tannin) would give a red which would not bear clearing. An excess of tannin gives shades which are very fast, but brownish."—*Chemical News.*

QUANTITATIVE DETERMINATION OF ZINC.

By F. BEILSTEIN and L. JAWEIN.

THE solution of the zinc, nitric or sulphuric, is mixed with soda till a precipitate is formed, and then with potassium cyanide till a clear solution remains. Platinum electrodes, conveying the current of four Bunsen's elements, are plunged into the liquid, which is kept cool if needful by plunging the beaker in a vessel of water. When all the zinc appears to be precipitated the electrodes are lifted out of the liquid, and the zinc is washed, first with water, then with alcohol, lastly with ether, and dried in an exsiccator. After weighing it is dissolved in hydrochloric or nitric acid, and the electrode, cleansed and weighed, is re-introduced into the liquid to ascertain whether the zinc has been completely precipitated.

TRANSFERRING LIGHTFOOT-BLACK FROM ONE FIBER TO ANOTHER.

By JUSTUS WOLFF.

LIGHTFOOT-BLACK dissolves in a strong solution of aniline hydrochloride in water, but not completely, with a deep greenish black coloration. The solution obtained in that way added to hot water dissolves with black violet color, and this liquid dyes cotton, wool, and silk gray. Even the Lightfoot-black on the fiber dissolves in a strong solution of aniline hydrochloride (produced by mixing 12 parts of hydrochloric acid of commerce and 10 parts of aniline).

About three years ago I dyed a large quantity of China-grass yarn with Lightfoot-black by soaking the yarn thoroughly in a strong solution of aniline hydrochloride and potassium chlorate, greening for about a week, and then passing through a bath containing small quantities of chrome and hydrochloric acid, washing, and drying. A small quantity of that yarn treated lately with a strong solution of aniline hydrochloride produced a dark greenish black solution, whilst the remaining fiber, after washing and drying, showed a dark greenish gray color. (Of course the fiber was corroded by the action of the aniline salt.) The greenish black solution mixed with water dyed cotton a beautiful bluish gray, and wool and silk a blackish gray, all standing soap very well, that on cotton even improving by treatment with soap. We see here that this coloring matter has by itself a very great affinity for the fibers, without being produced on the fiber as in the Lightfoot process.

The shades thus produced on wool and silk are not bright, proving that the Lightfoot-black process is unable to produce fine black shades at all on these animal fibers. The solutions obtained in the above manner contain too much acid and comparatively small quantities of coloring matter, so that it is very difficult to dye a deep black with. As far as I know, this is the first case of transferring Lightfoot-black from one fiber to another.

If the solution of Lightfoot-black in aniline salt solution is neutralized with caustic soda, diluted with water, and boiled till all the aniline is driven off by the water vapors, a grayish black powder remains in a light brown colored slightly alkaline liquid. The powder filtered from the liquid and washed on the filter with boiling water, consists of two different coloring matters, the one dissolving in boiling water acidulated with hydrochloric acid with a bright red color, dyeing cotton and wool of a dull red shade, which by washing with clear water turns reddish brown, and by soaping clear brown, and the other consisting of a dark blue-black powder, insoluble in neutral and acidulated water.

Here we have another proof that the Lightfoot-black consists of two coloring matters—a dark blue and a brown one.—*Chemical News.*

GREEN PIGMENT FROM CHROMATE OF BARIUM.

By THOS. DOUGLAS.

BARIUM chromate, which precipitates on the addition of a solution of barium chloride to a solution of a soluble chromate, is used to some extent as a pigment under the name of "lemon yellow." When strong sulphuric acid is added to this substance in the dry state great heat is developed, and it is colored a deep red from the liberation of chromic acid. If the mixture be now ground up in a mortar and heated to bright redness, the chromic acid is decomposed into the sesquioxide, which colors the mass green, yielding a pigment possessing considerable body. Assuming the reaction to be—

$$BaCrO_4 + H_2SO_4 = BaSO_4 + H_2CrO_4,$$

the amount of sulphuric acid requisite for the complete decomposition of barium chromate is 38·7 per cent. It is not advisable, however, to decompose the chromate completely; this would dilute the pigment too much with the barium sulphate; and, besides, the presence of a certain proportion of the yellow element seems to improve the quality of the color. Very good results are obtained by using 20 per cent. of sulphuric acid (o.v.)

From the great heat developed it might be rather dangerous to mix the dried chromate with strong sulphuric acid on a large scale. The better method of proceeding would probably be to mix the wet precipitate with acid of ordinary strength, and dry the mixture at a moderate temperature. In this way the chromic acid would be thoroughly incorporated with the other substances.

The pigment produced by the above process would doubtless possess great permanence and freedom from the objections arsenical greens are liable to. The cost of production ought to be moderate.—*Chemical News.*

ON A NEW VIOLET COLORING MATTER.

By M. PRUD'HOMME.

[Read before the Industrial Society of Mulhouse.]

[WE have already inserted a brief notice of this color, and now add further details.]

The coloring matter lately offered for sale by the firm of Baeyer, of Elberfeld, under the names "solid violet" and "anthracene violet," offers at once such striking analogies to galleine that one is naturally led to the comparative study of the principal properties of the two bodies.

Anthracene violet—the name may be preserved provisionally—is met with in the state of a brownish violet paste, like galleine or alizarine blue.

The solutions of the two coloring matters, either in alkalies or in other solvents, are almost identical in shade.

Both of them, if reduced with caustic soda and zinc powder, give yellowish brown solutions. The decanted liquors, if treated with an acid and then with soda, take in each case a reddish brown color.

The reduction in acid liquids presents analogous changes of color.

Cloth mordanted with alumina or with iron dyes up shades which differ little. The alumina reds have a more violet tone with anthracene violet than with galleine. On exposure to light this difference fades away.

Soaping at a boil discharges swatches dyed with galleine upon mordants of alumina and iron; under the same circumstances anthracene violet resists and is brightened. This property alone may serve to distinguish the two colors.

A passage through dilute sulphuric and muriatic acids has no permanent effect upon swatches dyed with either of the colors, the original tone being restored by alkalies.

Nitric acid destroys both irrecoverably.

Bleaching liquor degrades the shades of both in the same manner and in the same time, the colors finally disappearing.

Boiling lime water turns both to a very blue violet.

Among the mordants capable of fixing the anthracene violet the best results are obtained by means of acetate of chrome (chromic oxide, not acid), especially along with bisulphite of soda.

M. Horace Koechlin has already ascertained these conditions for galleine and coeruleine.

Steam violets obtained with galleine or anthracene violet stand soaping at a boil.

In certain conditions anthracene violet, fixed with acetate of chrome, may give rise to a blue. For this purpose the pieces prepared with oil, as for Turkey red, are printed with a color containing chloride of calcium along with acetate of chrome.

COLOR.

2 lb. 3 oz.	thickening.
7 oz.	anthracene violet.
2 1-5th fluid oz.	acetate of chrome, at 10° B.
4 2-5ths "	chloride of calcium, at 15° B.
4 2-5ths "	bisulphite of soda, at 20° B.

THICKENING.

105 oz.	water.
8¾ "	white starch.
17½ "	light calcined starch.
5¼ "	olive oil.

Steam for 90 minutes, wash and soap for half an hour at from 122°–140° Fahr.

On cloth not oiled this receipt gives a fine violet.

A galleine color applied in the same manner gives similar results, though the violet is less pure, and turns more to a violet.

Spectroscopic examination shows that the two colors belong to the same family.

Anthracene violet possesses a great tinctorial power; the product containing 5 per cent. of dry matter gives violets as intense as alizarine at 10 per cent.

It contains no nitrogen. It has lost one of the most important properties of galleine, that of being transformed into coeruleine by sulphuric acid.—*Teinturier Pratique.*